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Eight lectures
under the auspices of
the Department of Psychology in The College
of the University of Pittsburgh
delivered during February 23 and 24, 1951
in the Stephen Collins Foster Memorial

CURRENT TRENDS IN PSYCHOLOGICAL THEORY

WAYNE DENNIS
ROBERT LEEPER
HARRY F. HARLOW
JAMES J. GIBSON
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1951
UNIVERSITY OF PITTSBURGH PRESS

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THIRD PRINTING

PRINTED IN U.S.A. BY HERBICK & HELD, PITTSBURGH, PA.

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CURRENT TRENDS
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PSYCHOLOGICAL THEORY

DEVELOPMENTAL THEORIES

WAYNE DENNIS

OUR conferences on current trends in psychology, like all recurring phenomena, exhibit certain patterns or traditions. One element of our tradition, thus far, has been that I should introduce the speakers by means of some enlightened but innocuous preamble. When, this year, the members of the department of psychology chose psychological theory as the topic for our program, I was placed in a quandary. I do not feel competent or willing to prepare a preamble to psychological theory. An introduction to theory in psychology seems to me like a shadow of a shadow.

In this predicament, I decided to write, instead of a general introduction, a paper on current theory in a field which I feel I know, that of developmental psychology, and some months ago I boldly placed this title on the program.

This, I have found, was not an easy way out. Developmental theory is a difficult subject. We have had developmental theories, to be sure. Indeed, it would not be inaccurate to say that developmental psychology began with a theory—the theory of recapitulation. Child psychology, the most productive segment of developmental psychology, began shortly after the promulgation of the theory of evolution when all scientific minds were inflamed by this great conceptual achievement. The mentality of G. Stanley Hall was particularly inflammable, and the far-distant glow of man's origins emblazoned his imagination. The greatest outcome of his theory was the epitomizing statement "ontogeny recapitulates phylogeny." After the movement arrived at this apex of concise hypothesis, its decline was imminent. The evolutionary viewpoint had seemed to open up wide unconquered scientific vistas to child psychology. But on closer approach these beckoning plains proved to be inhabited only by unsubstantial figures and retreating will-o'-the-wisps. There was not a testable hypothesis in the entire landscape.

Following G. Stanley Hall, the next important proponent of a theory was John B. Watson. His theory that the infant begins with a few reflexes and three native emotions, and that all that happens to him thereafter is explicable as conditioning seemed very objective and scientific. The history of Watsonian behaviorism is so familiar that I need not review for you the gradual dissolution of his theory. Although it may now seem distant, it pervaded textbooks of child psychology until about 1935.

With the abandonment of the simple and splendid theories of Hall and Watson, child psychology seemed to lose its faith in hypotheses of large scope. It settled into a period in which it developed standardized tests for child performances, gathered data, and calculated an enormous number of means and standard deviations. In addition, much attention was given to the effect of various factors upon the I. Q., and the motion picture was praised as a way of stopping and preserving behavior for later examination. But neither the statistics nor the movie films revealed much that was not apparent to casual observation.

The child researchers of the past twenty years have widened our knowledge of childhood, but I believe it is generally admitted that there have been no startling discoveries. Indeed, while its contributions, particularly in the normative area, are recognized, there is today, I think, disappointment with the lack of achievement on the part of child psychology.

Barker and Wright[4] have recently evaluated the situation as follows: "The great weakness (of many studies of children) lies in their lack of conceptual schemes for systematizing the data and bringing them to bear on crucial psychological problems. The data are interesting in the way that a poll-taker's distributions are interesting; they provide intriguing facts that sometimes are of practical importance as well. The data would appear to bring important grist to the mill of child psychology, and the investigations have done a good job of sifting it; however, they have not done much grinding."

Even the quantitative output of research in child psychology has declined in the past decade. In the most recent edition of *Annual Review of Psychology,* Barker[2] calls attention to the fact that the number of scientific publications on child behavior in 1949 was only half as large as in 1938. Equally suggestive of decline is the fact that the membership of the Division of Childhood and Adolescence of the APA had fewer members in 1950 than it had two years earlier.

While the downward trend in research upon child behavior is doubtless due to several causes, it seems likely that one of them has been the relative paucity of theories which would stimulate research.

If it seemed that this would continue to be the case, there would be little reason to discuss current trends in developmental psychology. It is upward trends rather than downward trends which interest us. But despite the discouraging data just cited, I am optimistic. It is my belief that the postwar period has been particularly fertile in viewpoints, concepts, and theories which have implications for developmental investigations. As yet, they have had no great influence upon research activities, but I think conceptual trends have appeared which soon may bring about a renaissance of child research.

PSYCHOANALYTIC THEORY

The first movement to which I wish to refer consists of certain recent developments in psychoanalysis. Psychoanalysis, of course, is not new; it is more than fifty years old. But Sears,[37] summarizing its effects upon child research in 1943 could find relatively little to report. Are there reasons to think that the situation is changing? Let us see.

Psychoanalysis from the beginning was genetic, but genetic in a peculiar way. It was from the analysis of adult patients that Freud attempted to discover the developmental stages of childhood. His method was almost exclusively that of reconstruction. With the advent of the work of Anna Freud[12] and of Melanie Klein[17] direct psychoanalytic contact with children became more common. But even with child patients, much of the work of the analyst has dealt with reconstruction of the child's life prior to contact with the therapist.

Within recent years, however, a few psychoanalysts have undertaken direct observation of infants. It is noteworthy that these observations, in some cases, have had no therapeutic aim and no reconstructive aim, but have had as their purpose the establishment of sequences in emotional and personality development comparable to intellectual and motor sequences yielded by earlier investigators. Most of these studies have appeared in *The Psychoanalytic Study of the Child* of which five volumes have been published since its beginning in 1945.

It is my conviction that the beginning of direct investigation within psychoanalysis has enormous importance for a reapproachment between psychoanalytic theory and developmental psychology. It will give the two disciplines common referents which in the past they have not had. It will bring about a supplementation of our account of emotional development, which admittedly has been weak in comparison with our accounts of sensory, motor, and intellectual development.

This observational approach, I believe, is already having a profound effect upon the thinking of those who are participating in it. For instance, it seems to me significant for the future of psychoanalysis to find Kris[19] saying with reference to Melanie Klein, "While there are some who claim that they can establish irreconcilable contradictions between her chronological assertions and proven facts of child development, there are others who feel that any reprojection of mental content into earliest infancy (as attempted by Klein) is bound to be so vague that no single statement can actually be shown to contradict any purely observational data." Kris continues, "I have little doubt that the Kleinian hypotheses are so formulated that not a single one can actually be disproved by child observation." Here is a criticism of an analyst by an analyst which is like the criticism of a psychologist by a psychologist. I believe that such criticisms are likely to lead to revolutionary results in terms of more concrete and testable psychoanalytic hypotheses of personality development.

Another quotation from Kris[19] will, I believe, show further the increasing understanding between the analytic and the academic psychologists. He raises the following questions as worthy of study, "How can we, from observational data, predict the pathology that exists in a given child; how soon can we spot it from the child's behavior, from that of the family unit, or from the history of the mother and the child? Which therapeutic steps are appropriate to each age level and its disturbance, or to each typical group of disturbances?" To such questions, answers can be obtained. It is significant that they are being asked by people whose background is appropriate to their solution.

But it is almost inevitable that in the beginning the psychoanalysts acceptance of observation as a method will be uncritical. For instance, Spitz, who has been a pioneer among analysts in regard to observational studies, has made the following statement: "The emergence of consciousness in the human being has never been investigated by psychoanalysts. A number of psychologists, however, have done so. They were able to prove rather conclusively that consciousness, conscious perception and memory traces connected with these do not exist at birth and in the four weeks following birth. . . . The first signs of conscious perception . . . can be established toward the end of the second month. . . ."[39]

Since Spitz cites no references in this connection, I do not know to whom he credits these amazing discoveries. While it is flattering to have such important findings attributed to members of our profession, I do not know of any who would claim them. The criteria for the observation of consciousness in infants are still far from clear.

But if Spitz is generous with us, he is equally generous with himself. Among the discoveries which he indicates that he has established or postulated from direct observation of infants are the following:[39] The ego is present by three months of age, and the first rudiments of the reality principle are also present at this age. In the third quarter of the first year, narcissistic energy becomes free, perception becomes objective, and the mother becomes the libidinal object. He has also reported severe depression in infants.

When Spitz sees eight to ten months' old infants as seriously depressed, it seems quite likely that the usual checks on observer reliability should be made. We should not forget, however, that such checks are relatively new even in psychology. The hard-boiled Watson made errors in the interpretation of infant emotions and practically all textbooks accepted his interpretation without question for several years.

While it is probable that no progress can be made without error, we can hope for quick recognition of mistaken interpretations. Although many interpretations of child development are examined and abandoned, new facts will come to light in the course of this process. It is likely that current analytic hypotheses concerning emotional development will awaken a new interest in this area of developmental psychology. Even the extreme theories of Ribble,[34] which have recently been subjected to criticisms that seem to me to be devastating,[22, 33] may in the long run further our understanding of childhood. Certainly in view of the present-day concern with mothering, self-demand, and rooming-in, developmental psychology is now in a ferment which must at some time yield us wine or yield us vinegar.

CLIENT-CENTERED THERAPY

In addition to the psychoanalysts, there is yet another group whose primary interests are in therapy and whose proposed theories deal with the development of personality. I refer to the client-centered group. Their theoretical views have been concerned largely with the development of the self-concept. Hypotheses about the evolution of the self have been presented in recent years by Lecky and by Snygg and Combs. The most recent, however, has appeared in the current year and constitutes a most important chapter in Rogers' book,[36] *Client-Centered Therapy*. Rogers presents a theory of personality development involving nineteen propositions, relating primarily to the self and to the phenomenal field. It is impossible here to do more than offer the opinion that this theory will prove stimulating. Some of its aspects are very concrete and should be capable of experimental verification or refutation. Let us take Rogers' example of the situation arising when a child strikes his baby brother. The child may be told (not by a client-centered therapist), "You are naughty." This purely condemnatory response may be experienced as a serious threat to the self and can lead to a denial of the satisfactions that were experienced as a result of the aggressive behavior. But, Rogers says, there is a crucial difference in the consequences of striking the brother if the parent is able to accept the feeling of satisfaction experienced by the child, fully accept the child who experiences them, and at the same time accept the feeling that the behavior is undesirable. The child then need not reject as a part of himself his aggressive feelings towards his brother. Rogers proceeds further to outline the hypothetical outcomes of two types of parental behavior. This sort of theorizing occurs frequently in his book. Experimental testing of such a theory is difficult, not for logical reasons but for practical ones. The client-centered group, however, is skilled in making use of play situations as sources of data. It may be, therefore, that they will have means of investigation adequate to test their theoretical formulations. The contrasts between Freud's and Rogers' views of personality development are sharp, and these differences should be capable of resolution by research.

PIAGET

As a third source of theoretical stimulation in child psychology, I would propose Piaget. Piaget continues to produce apace. In a recent review, Aebli[1] lists twelve major publications by Piaget since 1936. Six of these are sizable books which have appeared between 1945 and 1950. Most of us will require as much time to assimilate these volumes as it seems to have taken Piaget to prepare them.

In Piaget's first four books,[23, 24, 25, 26] which were translated and which appeared in English from 1926 to 1932, he was concerned primarily with aspects of the thought of children between the ages of four and twelve years. His attention was given particularly to egocentrism, realism, artificialism, and animism.

In his more recent works,[27, 28, 29, 30, 31, 32] Piaget has expanded his consideration of child mentality in two ways. For one thing, he has extended his survey into the early life period, i.e., from birth to four years. He attempts to show a continuity of development from the reflex actions of the neonate to the abstract thinking of later ages. For instance, he notes that in the early months the infant discovers which objects can be placed in the mouth and which cannot. This practical knowledge, Piaget says, provides the basis for later classifications of objects in terms of functional similarity. Thus the origin of abstract groupings of objects lies in the child's commerce with these objects. Some objects are to suck, some are to throw, some are to tear. There is a continuous course of development from earliest actions to the first definitions by use. A similar continuity exists between the manual experimentation of the young infant and the mental trial and error and genuine inventiveness of the older child. These, of course, are not completely new ideas, but they are worked out by Piaget with unusual care and documentation.

In expanding his treatment, and in illustrating his presentation, Piaget draws primarily upon the detailed records of the development of his own children. His data, therefore, are of the biographical type; his books are unusual more for their interpretations than their data.

Besides enlarging his sphere of interest to include early infancy, Piaget has also encompassed new subject matter. Among the new topics treated are the development of the child's conceptions of time, space, and action, the child's ideas of number and measurement and of the conservation of weight and volume. Some of these topics are treated in two or more of his recent volumes. Many of them are included in a 1948 publication[32] on what Piaget calls the spontaneous geometry of the child. These works involve experiments and demonstrations with older children as well as observations of his own children during infancy.

While Piaget's contributions are developmental, they are limited to the childhood years. His interest in the human race seems to end at about age twelve. One cannot help wishing that he had put to average adults some of the problems about which he has quizzed the immature. It seems likely that he overestimates the objectivity and logicality of adult thought. At any rate, it would be interesting to extend his investigation into the adult age range.

It is going to be a very considerable task to evaluate each of Piaget's ideas. His first books have challenged the thinking and taken the time of several of us. The further volumes to which brief reference has been made here will influence the activities of child psychologists for many additional years.

LEARNING THEORIES

In general, learning theorists have given little attention to developmental problems. They have been interested in the general laws of learning; they have not been concerned with the developmental status of the learning organism. Munn,[21] in his review of studies of learning in children in the *Manual of Child Psychology,* indicates how little we know with certainty about the contrasts in learning between the child and the adult. While on the basis of intelligence test data we might assume that the child is inherently a slow learner, Munn's review suggests that in later infancy the slow learning of children may be based upon their lack of experience and upon their unwillingness to apply themselves rather than upon their neurological immaturity.

A recent book that will arouse repercussions in child psychology, as well as in other fields, is Hebb's[15] *The Organization of Behavior.* Hebb deals with many problems and presents many new ideas. The point most relevant to our present topic is Hebb's view that there are two distinct kinds of learning. The first is of the sort which occurs during familiarization with completely new material. He cites as an example his experience as a newcomer to the Yerkes Laboratory of Primate Biology. At first all chimpanzees looked alike. Only after a fairly long process of familiarization did similarities and differences appear. Once this background of experience was gained, the second kind of learning, the association of names or of bits of behavioral history with individual animals could go on at a rapid rate.

In the psychology of learning, primary learning, he believes to have been neglected. He notes the initial difficulties in organizing visual experience on the part of animals reared experimentally in darkness and then exposed to a normal environment[35] and on the part of persons blind from congenital cataract after their cataracts have been removed.[38] The early adjustment to the visual world is slow, but this adjustment is essential before the individual can make the quick associations and the mental manipulations of the visual environment which characterize the adult. While the examples which have been given are visual, it is believed that the principle is common to all kinds of experience.

Hebb proposes that the young child appears to be slow in learning because he is engaged in a difficult *kind* of learning. To the child everything is new, everything is unfamiliar, and the learning which he must do is of a kind which an adult moving in familiar surroundings is seldom required to undertake. Hebb cites the probable importance for child psychology in Harlow's experiments[14] with monkeys, which show the great changes in educability which can be brought about by repeated exposure to learning situations.

The ideas of Hebb suggest the possibility that by limiting our experiments with children to such performances as paired-associates, repetition of stories, repetition of digits, ring tossing, and stylus mazes, we have been blind to the most important aspects of learning in childhood. The socialization of the child, which we will discuss soon, may consist largely of the kind of perceptual structuring which Hebb calls primary learning.

Hebb's work may give a new emphasis to John Fiske's essay[8] upon "the meaning of infancy." Fiske held that the role of intelligence in human life, as compared to more instinctive adaptations in animals, required a long period of infancy to provide time for learning. Instead of accepting this interpretation, some of us have looked upon childhood primarily as providing time for maturation. Perhaps we have failed to evaluate the amount of basic learning which occurs in childhood.

SOCIALIZATION

The early work in child psychology had a strictly biological flavor. The study of the development of behavior was modeled after the study of the growth of structures. Structures emerge, change, unfold as if from some inner growth principle, and the mind was expected to grow in a similar fashion. This kind of thinking leads to the use of expressions such as the embryology of the mind.

In embryology the theories of performation gave way to the notion of epigenesis, the notion of interaction of organism and environment. The newest kind of embryology is experimental embryology, in which structures are treated not as fixed entities but as responding organs.

In child psychology the ideas of maturation and of inner sources of activity have long been prevalent. They no doubt contain a great degree of truth. But the child's growth must be related to the child's world.

The differences in the social worlds of children of different societies have received much attention in the past quarter of a century. However, the fruits of this interest have ripened to a marked degree only in recent years. The period since 1944 has seen the publication of Kardiner's[16] *Psychological Frontiers of Society,* DuBois'[6] *The People of Alor,* Kluckhohn and Leighton's[20] *Children of the People,* Erikson's[7] *Childhood and Society,* Thompson's[40] *Culture in Crisis,* and others which almost literally are too numerous to mention. These books differ greatly among themselves, but they have in common the general view that societies vary in the rearing of children and that differences between the adults of various groups are traceable in part to childhood care. The details of the relationships between child care and adult character will require much more work, and the methodological difficulties are great, but the work on this fundamental social problem has been vigorously begun.

Particularly noteworthy within the past decade is the clear recognition that American society itself contains castes and classes, the magnitude of whose differences are seldom realized by the layman or teacher or psychologist who ordinarily is well-acquainted only with his own social class. To be sure, investigators before Warner and Lunt,[41] Davis and Havighurst[5] had related child care and child behavior to various socioeconomic indices such as housing, income, and occupation of father. But the differences between a lower-class childhood and a middle-class childhood do not arise directly from differences in the fathers' incomes or from the presence or absence of plumbing. The more subtle factors of parental attitudes, expectations, and examples are the more important variables, and these were largely ignored, at least as class phenomena, until recently.

While child psychology has been invaded, and perhaps liberated, by anthropological and sociological concepts, the influence of these fields has not brought about any improvement in our methodology. The anthropological tradition of one anthropologist for one culture was a tradition arising from the lack of man power rather than from scientific standards. It is completely inapplicable to modern nations.

We clearly need methods to observe child life directly in complex societies. In this connection, an interesting recent development is the establishment of a social psychology field station by Barker and his associates.[3] This station, located in a small town in Kansas, may be the forerunner of numerous such stations throughout the world. It is somewhat patterned after the concept of a weather station. By contrast to it, our other child research stations are more nearly like temperature control chambers. Experiments can be done in them, but they give no information about the psychological climates under which children actually are reared. We need to know the real life situations which prevail in specific areas. A new discipline, which Barker has called psychological ecology, may be in the making.

A REVIEW

I have now summarized five sources from which new concepts and theories seem to be coming into child psychology. This has not been a complete list, nor are the listings mutually exclusive (analysts and learning theorists are interested in socialization, etc.). I believe, however, that the account is sufficient to carry the point which I wish to make. This is, that the past five years have been very productive of stimulation for new investigations relating to children. In this review, I have had occasion to refer to no less than twenty-four books bearing on our topic, all of which have been published since 1945. No previous five-year period has ever shown an equal record.

Let us examine the sources of the influences which I have described. Two sources of ideas have come out of therapy, the Freudian and the Rogerian. Theoretical and experimental psychology should probably be given credit for Hebb, although he is also connected with clinical psychology. The anthropologists and sociologists have introduced the fourth set of ideas. Of the five areas of theory, only one has arisen from a child psychologist, Piaget. If I am right, no important hypotheses have come from the largest group working with children, the educators, nor from another numerous group, that of social work. It would seem as if productivity in theory has nothing to do with the number of people professionally active in a given area.

FUTURE DEVELOPMENTS

In my title I indicated my intention to discuss theories in developmental psychology rather than theories in child psychology. This title was chosen because, it seems to me, that if a theory is valuable in relation to children, it should be valuable in life areas beyond childhood. If psychoanalytic theories of child psychology are correct, they have implications for adult personality and for therapy with adults. The same is true for Rogerian views. Anthropologists, in studying childhood, are attempting to understand adult society as well.

I should like to see the emergence of a genuine developmental or genetic psychology to replace our psychology of childhood. I think it is clear that a child psychology which stops at twelve years of age is apt to be rather sterile. This is where our books on child psychology *do* stop, and we have noted how few of our newer ideas seem to stem from professional child psychologists and educators. Lip service may be given to the idea that the child is father of the man, but we have not taken the idea seriously. Similarly, the so-called psychology of adolescence, in fact, deals with the junior high school and senior high school pupil, with no reference to the past or the future of this strange creature. When we have a psychology of old age, as we shall soon, it will probably contain no mention of life before sixty. If psychology does not change its ways, we shall leave childhood without a future; we shall enter senescence without a past.

To remedy this discontinuity, to see the whole course of human life *as* a whole—this is the task before us.

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CURRENT TRENDS IN THEORIES OF PERSONALITY

ROBERT LEEPER

IT HAS been a privilege to work on this paper. The psychology of personality is a live and virile field in psychology today, and one of unmistakable social significance. So, it has been a fascinating challenge to have this invitation from this conference to take stock of the main trends in personality theory and to try to predict what the most fruitful further developments might well be.

I am fairly sure that a great many psychologists would say that the outstanding current trend in personality theory—and, an exceedingly commendable trend—is one towards more objective concepts, concepts based on operational definitions rather than on something of a more subjective sort, and concepts based on experimental evidence rather than on clinical material.

I can sympathize with this view. There is a trend which can be described in these terms. But, I do not believe that the significance or true nature of this trend is that which is envisaged by most of those psychologists who see such great merit in this movement towards more objective, operational, experimentally-founded concepts. What I will propose, instead, is that this description of the trend in personality theory is merely an incomplete or partial way of seeing something more fundamental which has been happening and which will continue to happen. What I will propose is that this more fundamental development is moving in a direction which is opposite, even to that which is hypothesized by most of those who take delight in these objectivistic and operational tendencies.

To explain what seems to me the fundamental current trend, I might first go back and start with old Anton Mesmer. We might hesitate to cite him as the father of modern psychotherapy and personality theory when we think of all the phantastic elements in his procedure and interpretations. But, if he is not the father of modern psychotherapy, he is at least the grandfather or great-grandfather. The work of Liébault, Bernheim, and Charcot may be said to have grown indirectly out of his work; Freud's work is more or less an outgrowth of their work; and current psychotherapy and personality theory surely are tremendously indebted to Freud. So, let us start with Anton Mesmer as he donned his long, magician-like robe and placed his patients so they could grasp the metal rods extending from his tub or baquet filled with iron filings which he had permeated with his "animal magnetism." It was an impressive scene, especially when it was known that this situation produced violent convulsive "crises" in many of the patients and, in a fair number of them, seemingly produced complete cures from lameness, paralyses, blindness, and other apparently medical disorders.

There are four things which I want to note about this grandfather of modern work on personality. *First:* He concerned himself about tangible symptoms, with things which he believed were actual organic ailments. In so far as we would imagine, today, that he had any successes, we would attribute these to the fact that his patients must have included a number of cases of hysteria. But, even if he had known this fact and had excluded all other types of patient, it still is true that he would have been taking the form of neurosis whose symptoms are more tangible—more easily recognized—than those of any other personality maladjustments.

Second: Even though he was a pioneer worker, Mesmer did not start with personality phenomena which are common and important in most of our everyday life. He started with dramatic and extreme material, even though it was relatively a rare sort of effect.

Third: The type of explanatory theory which he developed was a theory which stressed causes which were tangible, just as the symptoms were tangible. The factor to which he credited his cures was imaginary, of course. There was no direct evidence that some animal magnetism flowed from his hands into the tub and from there into the bodies of his patients. But, it was easier for Mesmer to develop and believe this physicalistic explanation than some theory in terms of psychological factors.

Fourth: Even though Mesmer did not realize this fact, his procedure actually used some relatively intangible factors of suggestion, motivational satisfaction, and so on. So, any scientific study of what he was doing would have had to identify and formulate the laws of some subtle, relatively intangible factors which Mesmer knew nothing about.

I might even add a fifth feature. I don't know whether this was actually characteristic of Mesmer. But, I do know that many psychiatrists and psychologists since Mesmer who have resembled him on these other four scores *have* shown this fifth trait—that they have tended to arrogate to themselves the claim to real tough-mindedness, real scientific spirit, and so on, just because of the fact that they phrased their explanations in terms of relatively tangible causes and effects rather than in terms of more subtle influences as well.

Since Mesmer's time, what has been happening? The main trend which I see over this whole century and a half, and the trend which I see as the most probable one on into the future as well, is a trend away from such exclusive emphasis on tangible factors and towards an increasing appreciation and use of the relatively intangible. And it is a trend, in consequence, away from emphasis on what is relatively infrequent towards emphasis on what is much more common and socially important.

This has been a slow, gradual development. For instance, early in the first World War, many physicians and psychiatrists were doing little better than Mesmer had done a century before. Confronted with cases of hysterical symptoms, they still had not learned to use the subtle cues which could indicate the difference between organic and functional disorders. The explanation they tended to give was that these cases were products of widespread hemorrhage in the brain from the pressures from high-explosive shells. They had no direct evidence of such hemorrhages, any more than Mesmer had of his animal magnetism. But, it was easier to believe in simple, tangible causes, even if they had not been demonstrated, than in more subtle factors.

We have moved some distance beyond that viewpoint today. But it still is true that the veterans administration hospitals are dedicated to those men who were left with relatively tangible symptoms from the two World Wars. We have not conceived, for instance, that it might be even more important to provide therapeutic aid to those men—some of whom are found in fairly high places in the country—who were left by their war experiences with the conviction that the one main way to handle international relations is by military force. It still is true that tangible aspects of personality, like hysterical disorders, phobias, and anxiety states are the symptoms which we can appreciate, even though some such influences on social attitudes may be much more threatening to the general social welfare. But the trend in personality theories is towards a recognition of the latter.

If there is such a movement, however, from the relatively tangible to the relatively intangible, it is nothing peculiar to psychology. The whole growth of human knowledge, and the whole development within each science, would illustrate the same trend. Back in 1543, for instance, two books were published which marked the beginning of modern science—the book by Copernicus on the solar system, and the book by Vesalius on human anatomy. Both of them started with what was tangible. Consider Vesalius, for instance. He did not begin with psychosomatic disorders. He did not dream about iodine deficiencies as causing goitre and cretinism, nor about lack of vitamins as causing scurvy or pellagra. He did not dream about microscopic organisms as causing smallpox or cholera. In his work, medical research started with the most tangible part of its field.

Why doesn't each field continue this stress just on tangible things? If this interest in tangible factors is most natural at first, why doesn't it continue? The answer is fairly clear. Both in technology and in science, the goal of our work is to learn how to make predictions, and, if possible, to get the means of control, in as wide a diversity of situations as possible. At first we try to achieve such predictions and control by attending just to relatively tangible variables. But, our work fails at too many points when we proceed just in this way. We are forced towards a recognition of relatively less tangible influences because only in this way can we realize the objectives of our work. Ivan Pavlov, for instance, did not eagerly welcome the idea that gastric secretions could be influenced by learning. He had been convinced that digestive processes were governed solely by innate reflex mechanisms. But, when he watched the facts under certain conditions, he could not account for them with his original theory. In the same way, Gordon Allport originally phrased his principle of the functional autonomy of motives in terms of one of the crudest possible variables. He said that a new motive or interest always would be produced if a person was compelled, by whatever means, to go through the outward performance of such actions as ordinarily would have expressed such a motive. This was an amazing theory in a psychologist who generally is so inclined towards interpretations which emphasize relatively intangible factors. This explanation was one which Gordon Allport found quite unacceptable as he considered more cases. But, his experience illustrates the point that we tend to think of tangible factors first, and shift to a recognition of relatively less tangible factors only as we are forced to by the facts.

These words "tangible" and "intangible" have been getting a lot of use in this paper. They had better be defined.

The old meaning of the words suggested a very literal dichotomy. Those things were tangible which could be perceived by the sense of touch; those things were intangible which could not be thus perceived. By this definition, even the color of a paper or the sound of a buzzer are not tangible things.

For our use, however, we need another definition of tangibility, one which has tended to develop out of that original definition. It is a definition which assumes a continuum. It is a definition which equates tangibility with perceptibility. Those things are relatively tangible which are relatively easy to perceive or recognize, no matter by what sense. Those things are relatively intangible which are relatively subtle, elusive, or imperceptible, hard to grasp mentally, hard to identify or measure.

It is not a question, then, as to whether things are material or immaterial. Bacteria, for example, are quite material things. But, before the development of the microscope and of staining methods, bacteria were imperceptible. On the other hand, the beauty of Gothic cathedrals was immaterial—something which did not exist except as a product of psychological processes. Yet, it was more perceptible, and hence more tangible in the fourteenth century than were the bacteria of the fourteenth century.

It is interesting, in the light of this definition, to consider the material with which the field of experimental psychology began. In the 1880's, psychologists were working with after-images, illusions, psychophysical judgments, and the like. Intangible material? In a degree, yes, because, in our ordinary life, after-images, for instance, are not ordinarily observable. But, in some other senses, these phenomena are away over towards the tangible end among psychological effects. You can take anyone, educated or uneducated, forty years old or ten years old, put him in a situation favorable for the production of negative after-images, ask him what he sees, and he will report the stock effect. If you give him a series of other colors against which to match his after-image, he can even indicate fairly precisely just what after-image he sees. What is more, the independent variables which were involved in such experiments were also relatively tangible. Even in the case of the introspective psychologists, therefore, experimental psychology began with relatively tangible material, just as medical research had begun in the same way in the anatomical studies of Vesalius.

I have been speaking in rather broad terms. Let us narrow our discussion, now, to the more specific field of personality theory. Is there any indication in *this* field that there is some trend from the tangible towards the relatively intangible? And, if there is such a trend, is there any indication that it involves a shift from the relatively infrequent and extreme towards material which is much more common and hence much more important?

To answer these questions, let us look first at the problem of what phenomena, or what effects, are most emphasized by many of the theories of personality.

In the early 1800's, as we were saying, Mesmer was working with cases of hysteria, even though he did not recognize them as such. At the same time, the earliest psychiatrists were working with the psychoses—the hardest type of case to try to treat. In other words, the work started with cases which were relatively rare, but which were the most tangible or perceptible cases.

At the close of the last century, much the same still was true. Freud thought of the psychoanalytic method, at first, as something suited solely for the treatment of hysteria. Pierre Janet in France was occupied with the same sort of case. Morton Prince and Boris Sidis in this country were writing about cases of dissociation, multiple personality, and so on. It took some time before the psychotherapists developed the capacity to identify and work in terms of compulsive and obsessive disorders, anxieties, and severe guilt feelings. Still further, within psychoanalysis, it was not until the 1920's that some earlier suggestions of Freud were picked up by Karl Abraham, Wilhelm Reich, and Sandor Ferenczi, and that some careful attention was given to the question of "characterological types." According to the fine summary recently given by Clara Thompson, these psychoanalysts did not regard this matter of personality patterns as a fundamental question, at first. According to their thinking, some of the most troublesome resistances to psychoanalysis came from "character resistances" within their patients. So, they felt that they had to learn to recognize such differences of character in order that they could explain to their patients, early in the course of therapy, the reasons why the patients were having difficulties in trying to follow the rule of free association, or why they were having difficulty in accepting some of the interpretations of the therapist. The psychoanalyst, for instance, would point out to one person that he seemed to feel threatened at any point where he might need to accept help from another person—that, even though he had come for psychotherapy, he had the implicit assumption that he ought to be able to work out every point for himself. With another patient, the psychoanalyst would point out that the person believed, implicitly, that others would feel pity for him, and abstain from attacks on him, only as long as he continued to prove that he was suffering and handicapped.

The idea back of such attention to character traits was that, when the therapist thus analyzed the sources of resistance in the patient, the patient would then be freed to go ahead on the really basic work of discovering, by free association, some other types of unconscious material. The hypothesis was that the *crucial* unconscious material was repressed instinctual material, not these personality patterns as such.

Many psychoanalysts still think in these terms. But, with many others, it is more and more true that a psychoanalyst would be likely to make this sort of comment: He would say that the trouble with most patients is that they start treatment with the idea that what is wrong with them is their symptoms, or some narrow cause which is producing these—but, that the patients have to learn, to their chagrin, that the trouble lies in the whole over-all pattern of their lives, and that it is this that has to be changed.

In some ways this is not a new idea, because Alfred Adler and Carl Jung were emphasizing this same notion several decades and more ago. But, it has been coming out with renewed insistence in the work of Karen Horney, Erich Fromm, Harry Stack Sullivan, and others. This emphasis is *less* apparent in the writings of the more nearly orthodox Freudians, but even there the trend shows up in some ways. Thus, it is instructive to study the index of Franz Alexander's 1948 book, *Fundamentals of Psychoanalysis.* In this index there are fourteen references to the term "super-ego," sixty-one references to the term "ego," and only two references to the term "id." In Alexander and French's book on *Psychoanalytic Therapy,* published in 1946, the terms "id" and "instinct" do not appear at all in the index.

The subject matter with which our theories of personality have been concerned has been changing in another main respect, too. In the book on *The Psychology of Adjustment* which L. F. Shaffer published in 1936 there were extensive discussions of repressions, phobias, adjustment by ailments, persistent nonadjustive reactions, and so on. Even though the title of the book was *The Psychology of Adjustment* rather than *The Psychology of Maladjustment,* there was practically no discussion in it of healthy personality patterns. The same thing is true of the great range of psychoanalytic writings. Even Karen Horney, despite her many shifts from older psychoanalytic thinking, still writes with this same mainly negative emphasis. In her *Neurosis and Human Growth,* published last year, Horney says that our work with personality should aim at the ideal of " . . . the liberation and cultivation of the forces which lead to self-realization." But then, as indicating the problems around which her book actually is centered, she continues with this significant statement, "I hope that this book, by a clearer exposition of the obstructing factors, may . . . help towards this liberation."

This is a strange thing. The practical goal which we want to serve by a better understanding of personality is partly a positive goal, of attaining worth-while personality characteristics, and not merely a remedial goal. But, in the past, our theories of personality were stated largely in terms of the various aspects and causes of malfunctioning. The personality characteristics and processes of unusually well-adjusted persons, and even the personality characteristics and processes of persons of average adjustment, were passed over as though they called for no special technical study. Even in Erich Fromm's fine volume in 1941, *The Escape from Freedom,* there is a detailed examination only of the sadomasochistic or authoritarian personality. The complementary concept of a democratic personality, as we might call it, is hardly touched at all.

Why has there been this one-sided, negative emphasis? I think there are two main types of answer which have been given. On the one hand, it has been said that this negative emphasis is desirable, or at least acceptable. On the other hand, it has been said that this negative emphasis, even though undesirable, is inevitable. Let us look at these arguments.

Among the psychoanalysts, a typical view was expressed by Nina Searl in 1936. She said:

In this chapter we have talked much about anxiety, and insecurity, about disturbing and obscuring factors; scarcely at all about situations in which these are absent and in which children and adults have a happy freedom of question and answer. The fact of this omission illustrates the belief implicit in psychoanalytical practice, that we need only to deal with the impairment of function, with that which impedes, deflects and exaggerates it, and to prevent and remove such impediments, for it to be spontaneous and accurate.

That seems plausible in some ways. But in some other ways it does not. No one would expect to become a good violinist, or a good basketball player, or a good research worker merely by dealing with what might impede or deflect or exaggerate various functions within himself. Instead, he would need to have some good understanding of the possible fine skills within such areas, and he would have to learn how to develop those skills. In all of such things, we know that a primarily positive emphasis is needed. They are cases of fairly complex functioning, of course. But so is personality; and, with regard to personality, I think it is just as important that we should learn about efficient, successful functioning, and not just about malfunctioning.

At least some among the psychoanalysts might agree with this. But, many of them would say, it is not feasible to get a really deep knowledge of well-adjusted personalities by studying them directly. Only a maladjusted person, they contend, will be motivated deeply enough to undergo the intensive and often emotionally painful work which is required to get really worthwhile data on the psychology of personality. Therefore, they say, all of our best knowledge about personality must come from psychotherapy, and must be centered on the unfavorable types of development.

As a description of our work in the past, this is what we have seen. But the trend now is definitely towards a use of a wider range of research situations than merely the therapeutic interview; and, as a by-product of this change, the trend is increasingly towards emphasis on the characteristics and sources of healthy personality, and not merely on personality maladjustments. Within psychoanalysis itself, I think the most significant development lies in the work of Margaret Ribble and Renée Spitz. Some psychologists have expressed the impression that their work is a mere rehash of traditional psychoanalytic notions. On the contrary, their material has revolutionary implications for psychoanalytic theory. For one thing, it suggests that personality development is very heavily dependent on learning, even in early infancy, rather than mainly dependent on maturational influences, as per Freudian tradition. For another thing, their work suggests that our theories of personality ought to be based partly on studies of subjects in their real-life environments, and by nonverbal methods which are amazingly reminiscent of those of John B. Watson and Arnold Gesell, rather than just by a use of therapeutic methods with older children and adults.

The recent volume on the *Authoritarian Personality* by Adorno, Frenkel-Brunswik, Levinson, and Sanford is an especially impressive instance of this trend away from sole reliance on psychotherapy and on maladjusted personalities as sources of research material.

The psychologists who conducted this study were highly sympathetic with psychoanalytic views. It was a practical concern with social prejudice which gave rise to the study. And yet, in spite of these facts, the study was concerned almost as much with those who were unusually free from prejudice as with the highly prejudiced subjects.

The California Adolescent Growth Study, under Harold E. Jones, has been another major step away from using just maladjusted persons as sources of data on personality. The same thing is true of the work of A. L. Baldwin and his colleagues at the Fels Research Institute at Antioch College in their studies of the influence of home backgrounds on personality characteristics of children. Abraham Maslow also has contributed a very notable paper recently, one which I believe ought to be followed up by many other studies with the same emphasis. He located first a very selected group of persons whom he regarded as having been extraordinarily successful in achieving the goal of "self-actualization," and then he devoted his efforts to trying to identify the characteristics of this group. We have had hundreds of studies, previously, which have tried to do this same thing with criminals, with psychopathic personalities, with homosexuals, with schizophrenics, and so on. It is high time that we started to develop our work in this other direction as well!

In a lot of ways, therefore, the subject matter of the psychology of personality has been changing and is likely to continue to change. There is one source of this change which we ought to mention now. You may already have noticed it from some of the examples which I used earlier. What I refer to is the fact that some of the changes in personality theory are occurring because of some considerable shifts in the character of the group which is doing the theorizing about personality.

In academic psychology, twenty years ago, there was very little work on personality, either from a research angle or from the angle of training therapists. The great bulk of work on personality was the contribution of psychoanalysts and other psychiatric workers—primarily a medically trained group. There were few of these clinical workers who knew much about psychology except as it was derived primarily from therapeutic work and except as it bore primarily on therapeutic work. But this situation certainly has been changing. Both as regards research and therapy, the field of personality is more and more becoming a major field of interest of psychologists.

What bearing does this have on our hypothesis that there is a trend towards a stress on relatively less tangible factors? Does this shift of personnel fit in with this, or lie in opposition to it?

The situation, I believe, is far from simple in this respect. In some ways, the shift conforms with the trend that I have described. The academic psychologists have had technical training in dealing with a considerable host of psychological phenomena. They are trained in thinking about them, trained in research techniques related to them, and so on. Consequently, the academic psychologists often have some understandings and techniques which make them appreciative of, and skillful in dealing with, some issues which the typical psychoanalyst or psychiatrist must deal with only in terms of the concepts of everyday life. Accordingly, the major contributions in some areas of personality interpretation are shifting from psychiatry to psychology. Take the Rorschach test, for example. It originated in psychiatry. But, psychologists are so much more familiar with the research methods required with psychological tests, so much more familiar with statistical techniques, techniques of validation, and so on, that the Rorschach work has tended to become primarily a field for psychologists, not for the workers in whose area the test originated. The same thing seems to be happening to some extent with regard to psychotherapy. There are not many workers yet who have been trained in the methods of nondirective therapy, for instance. And yet, small as this group is, the work of Carl Rogers and his students has been making history in the field of psychotherapy through an introduction of effective research techniques into a situation which always before was regarded as too intangible for scientific study. So, I think there are some respects in which the academic psychologists already have facilitated the trend from the tangible towards the intangible.

But, in certain other respects, there is a risk that the academic psychologists will lose some of the ground which has been gained. We need to give some real thought to two main criticisms from the psychoanalysts, particularly. In the first place, they say, one great trouble with academic psychologists is that they tend to take behavior and conscious experience too much at their face value. Consequently, they say, the entrance of psychologists into the field of personality is bound to mean a loss of appreciation of many of the intangible factors and relationships appreciated by "depth psychologists." In the second place, the psychoanalysts say, the academic psychologists fail to appreciate that a part of the necessary technical preparation required for research in the field of personality is an emotional preparation, or personality preparation, so that the worker can have the hardihood to face various facts which would be deeply disquieting to most laymen.

I feel that both of these points are profoundly important. Thus, it is true that we psychologists have neglected the processes which permit an individual to express the same underlying motives in quite divergent ways—even sometimes in seemingly opposite ways—under different circumstances. Among the stimulus-response or S-R psychologists, Hull's concept of habit-family-hierarchies is a partial attempt to explain such cases. But, this concept has remained virtually unmentioned by Hull's group ever since it was first proposed in 1934. Such psychologists as Lashley and Tolman have put more stress on the phenomena of equivalence of response; but, even here, there has been little exploration of the richness of those processes which show up in the field of personality in such effects as displacement, symbolization, sublimation, and projection. We have shown the old tendency to feel more confident about our work when we could restrict our explanations to factors of a relatively tangible sort. Hence, we have neglected one of the phenomena which, as Gordon Allport points out, is one of the most widespread phenomena of personality.

A lot of psychologists would scoff at the second point, that it takes some special emotional preparation to work effectively in the field of personality, even as a research worker. Some of them would phrase it in this way, indeed: that the point is, rather, that no one can believe such fantastic notions as the psychoanalytic theory unless he has been emotionally indoctrinated while his technical knowledge of psychology was still undeveloped. Such skeptics are pointing to a real danger. It *is* possible to build up emotional loyalties which will sway one's intellectual processes too strongly. But, by the operation of the same type of factor, it also is true that the psychologist or psychiatrist who has somewhat of a precarious personality adjustment within himself, as admittedly many of us do, will tend to close his eyes to some data, or refuse to consider some material, which he would be able to view more objectively if he were emotionally more free. The psychological processes of the research worker are part of the instrument by which he makes his observations and works them into conclusions. The emotional and other personality characteristics of the research worker, in turn, are part of what determines the quality and objectivity of these psychological processes, particularly in this area of personality study.

At least in some ways, therefore, I think it is true that the training of academic psychologists, in the past, has not taken account of certain relatively intangible factors which must provide part of the equipment of the good worker or good thinker in the field of personality. I think there will be some temporary setbacks, therefore, as the theories of personality come to be a product more extensively of psychologists rather than of mainly psychiatrically trained persons. But there is also a trend in psychology towards admitting these several shortcomings which we have, and I think that the future will see even more of an effort to deal with these several points. For better or worse, at any rate, we have to recognize that personality theory is now the concern both of psychiatrists and of psychologists. The future holds the possibility—or, I would say, even the probability—that personality theory will come to be mainly an area of contribution from psychologists, just as the main contributions to most problems of physiology, genetics, endocrinology, and neurology now are made by specialized technical workers rather than by the field of medicine where these types of work originated.

There are two subareas of the field of personality where we can see this same kind of uneven seesawing back and forth between tendencies to stress only tangible factors and tendencies to stress intangible influences as well, and yet where we can also see this same over-all trend towards things which were imperceptible before. Even in Freud's early work, some real progress towards recognition of intangibles was made through seeing that so many phenomena of personality are affected by motivational factors which had not been appreciated previously. But, it also is true that the early Freudian notions of motivation were strongly towards the tangible end. The chief stress was on the sex motive—surely one of the most tangible of human motives, both psychologically and physiologically. Some attention was given to anxiety, but the early psychoanalytic theory of anxiety treated it virtually as a toxic by-product of sexual deprivations. Only from about 1920 on did destructiveness or hostility come to be recognized as a basic human motive. Only a bit later was anxiety recognized as a product of the individual's perception of dangerous forces within himself. These were real shifts towards recognition of less tangible factors. The more recent psychoanalytic workers have carried this trend even further. Karen Horney, for example, points out that we cannot understand cases of seemingly intense sex drive in terms of the sex drive as such. We must understand such cases, instead, in more subtle terms of the individual's seeking to gain security, or to gain affection, or perhaps even to humiliate and exploit other persons. The Freudians have felt that other people take psychological processes too much at their face value. Horney has been showing that even the Freudians were doing this to some extent themselves in their interpretations of motivation.

It is interesting to trace the development of concepts of motivation within academic psychology, too. In the early decades of experimental psychology there was hardly any attention to phenomena of motivation. They were too difficult to study, too intangible. Then, as animal psychology developed, psychologists were forced to give some thought to problems of motivation and to problems of reward and punishment. When psychologists started to work with cats and rats and monkeys, they could not take the factor of motivation for granted. It had been possible to do that when graduate students were the main subjects in experimental studies; but the cats and rats and monkeys were so lacking in scientific spirit that the animal psychologists were forced to recognize a factor of motivation which had always been an element in psychological research, but which had never been appreciated.

When they began to think about matters of motivation, however, the animal psychologists thought almost entirely in terms of physiological motives or bodily drives. These were the motives which were easiest to see and easiest to control. With regard to emotional processes, on the other hand, the animal psychologists found these a nuisance. All that emotional processes seemed to do in their subjects was to interfere with the progress of experiments. Thus, in a maze experiment, if you accidentally pinched a rat's tail by closing a retracing door too soon, the rat might be so fearful on subsequent trials that he never would go past that spot and never would learn to run the maze. So, the psychologists said, emotional processes have a function which is opposite to that of motives. Motives organize and direct and steer behavior; emotions, on the contrary, are basically disruptive and disorganizing. This idea spread. Not only among animal psychologists, but among academic psychologists rather generally, as I pointed out in a survey of this matter several years ago, there has been a widespread tendency to see emotions as merely disruptive affairs.

In the first edition of his *Physiological Psychology* in 1943, Clifford Morgan pointed out very clearly a number of reasons why emotional processes ought to be thought of as having a motivational character. In 1948, in the review which I spoke of, I repeated the argument all over again, ignorant of the fact that Morgan had done a good share of the job earlier. Yet, in the following year, Donald Hebb, in his book on *The Organization of Behavior,* argued more strongly than ever that the concept of emotion as essentially disruptive is *the* correct view.

It seems that the concept of emotions as motives, rather than as subversive agents, has a hard time finding a foothold in academic psychology. Nevertheless, I feel pretty confident in saying that the concept of emotions as disruptive is tending to be replaced in academic psychology by the concept of emotions as primarily motivational. It is noteworthy, for instance, that some of the S-R psychologists, such as Neal Miller and O. H. Mowrer, have come to stress this view. Partly on the basis of some ingenious experimental demonstrations, they are insisting that fears can be learned and then can operate as drives which permit the learning of some other habits. They are proceeding cautiously, maybe even timidly. Thus, in Dollard and Miller's current book, *Personality and Psychotherapy,* almost the only motives which are discussed are the old psychoanalytic trio of sex, hostility, and anxiety or fear. Nevertheless, the book is a significant indication. It points to the fact that the idea is really taking root in academic psychology that motivation needs to be understood, not just in terms of relatively tangible factors such as the more obvious bodily drives, but also in terms of the more subtle influences of emotional processes as well.

As our understanding of motivation increases, it also is true that we are coming more and more to see that motivation must be understood in terms of a "goal-directedness" of behavior. This idea was rejected for a long time by most psychologists because they felt that any notion of "goal-directedness" must inevitably imply a teleology, a determination of present behavior by some factor existing only in the future. It did not seem possible to believe, as Tolman urged, that the concept of purposiveness, or goal-directedness, does not assume such a determination by future events, but assumes only that there is some regulating mechanism within the organism which represents that goal which might possibly be attained in the future. This seemed like such dubious scientific thinking! But it is a hard thing for a psychologist to adjust the thermostat on his furnace first, so that the furnace will work in a goal-directed fashion, and then go back and write papers about the objectionable, teleological character of any theories which assume a goal-directedness in psychology. The increasing emphasis on homeo-static mechanisms in physiology, and the increasing development of servo-mechanisms or negative feedback mechanisms in industrial and military devices, will surely someday give the psychologists the courage to stress the idea that animals and human beings also show the same basic phenomenon in their psychological processes.

Within the current theories of personality, this notion of "goal-directedness" is appearing most strongly in Karen Horney's increasing emphasis on the "idealized image" or "idealized self" as a factor in neurotic personalities. There are parallel developments in other parts of psychology. It might make Kenneth Spence a bit unhappy if he realized that his recent emphasis on secondary motivation, secondary reinforcement, and so on, is making him a close intellectual kin of the neo-Freudians and of the earlier theories of Alfred Adler.

The basic similarity is there, however. Increasingly, I believe, we are going to see a major emphasis put on the concept which Adler emphasized some decades ago, that the motivation of the individual must be understood rather considerably in terms of the goals which the person has come to regard as the essential and indispensable objectives of his life.

On the matter of the motives involved in personality, there is still another trend towards thinking in terms of rather subtle factors. The older methods of psychotherapy all assumed that a neurotic patient could be motivated to do the painful, anxiety-arousing work of therapy only if he had rather tangible motives. He had to be driven by the suffering from his symptoms. He had to form a deep emotional attachment to his therapist so that he would work partly to please this parent-surrogate. He had to be prodded and pushed by the psychotherapist, as by insisting on a "fundamental rule of free association," or else he would always skirt the repressed materials which he ought to explore. In the work of Otto Rank and Frederick Allen, however, some new methods of therapy were explored which rested partly on some changed interpretations of human motivation. Their new methods were founded on the theory that part of human motivation is an "urge to grow," an "urge to achieve independence," or the like. This sounds awfully vague and intangible. But, when Carl Rogers sums up the main conclusions of the past decade of research and practical experience with nondirective psychotherapy, he emphasizes this same motive. Rogers believes that this "urge to grow," or "urge to mature," is a main variable which they encountered in their work, and that it is a major demonstrable factor in human personality in general. I think the same thing will come to be said about a number of other emotional motives—for instance, that there is an "acquirable drive" of affection for others which is something other than just a craving for protection or a matter of conditioned sexual reactions. Our whole current social system tends to be founded on the assumption that human motivation is only a matter of craving for very tangible values; but, as psychology moves on, there is the possibility that our technical concepts of motivation will stress quite different factors, and hence have profound implications for social organization and social action.

Just as theories of personality motivation have shown an uneven but general trend towards dealing with more intangible materials, so also is this seen in a next field. Within the last few years, we must surely say that an outstanding trend in the theory of personality has been the effort of experimental psychologists, and even of animal psychologists, to link up their theories of learning with theories of personality and of psychotherapy. Particularly notable is the work of Guthrie, Neal Miller, and Mowrer. It is true that these men are S-R psychologists, and it is true that the S-R psychologists have a sort of species characteristic of preferring to think in terms of "tangible" factors rather than of relatively intangible factors. Yet, even in the work of these men, we have some movement towards the latter. Within Freudian theory, it is remarkable how little explicit recognition may be found of learning. The personality development of the child is portrayed mainly in terms of a succession of maturationally-determined stages. The mention of learning is brought in incidentally and grudgingly, if at all. The reason for this, I believe, is the heavy medical background of psychoanalysis. These workers had more of a feeling of assurance when they were talking about maturation, and about such biologically-determined motives as the sex motive, than when they were talking about learning, which obviously would have to be thought of in more psychological terms.

Even Karen Horney, in her current book on *Neurosis and Human Growth,* declines to view learning as an important problem in personality theory. At the beginning of her book she considers the possibility that personality might be a product of learning, and says that a child will

> . . . learn to cope with others in one way or another and . . . probably acquire some skills. But, she continues, there are also forces in him which he cannot acquire or even develop by learning. You need not, and in fact cannot, teach an acorn to grow into an oak tree, but when given a chance, its intrinsic potentialities will develop. Similarly, the human individual, given a chance, tends to develop his particular human potentialities.

She, of course, is correct in saying that an acorn cannot develop its intrinsic potentialities by learning. But, her analogy is too loose when she concludes that, since wholesome personality development needs likewise to be a development of "intrinsic potentialities," learning would have no part in this! In her descriptions of personality patterns seen in maladjusted persons, Horney has made some remarkable contributions towards the recognition of many factors too subtle for previous workers to have traced; but in her refusal to consider learning as a process which ought to be recognized explicitly by a personality theorist, she is pulling in the other direction. It suggests that, even if Miller and Mowrer are on the side of the "tangibilists" in some respects, in some other respects they are part of the cavalcade towards the intangible end of our continuum.

I must confess, though, that I think that the enrichment of personality theory by learning concepts has not gone very far yet. The big trouble, it seems to me, is that the main efforts have been made by S-R theorists, and that, in turn, the S-R theories have been meager, undernourished theories. They have been so afraid of eating anything except the most tangible looking kinds of food that they are suffering from severe qualitative food deficiencies.

But, there is a richer material which could be used. Ann Magaret, for instance, has called attention to the fact that Harlow's experimental demonstrations of learning sets might be applied very successfully to the problem of understanding more adequately why psychotherapy is able to help out the adjustments of a person in his real-life situations. The work on hypotheses and learning which was done by David Krech in one of his earlier incarnations is another example of some rich experimental work which ought to be picked up again and explored further as related to personality. On the side of the psychotherapists, especially such as Rogers, Horney, and Adler, we have been finding an increasing emphasis on "concepts of the self" and the like. Krech's early work and other experimental work on concept-formation could give the learning theorists something which would throw more light, perhaps, on these "concepts" of the psychotherapists. Or, to take a third example: Even though Hebb's psychoneurological theory of learning is rather speculative, I think it has some profoundly stimulating implications in it. It may be that such studies as his will help us to understand how human life can become such profoundly different things under different learning conditions.

There is some rich material, therefore, which the S-R learning theorists do not use when they write about personality. But, even so, within their own theory, this progression towards the recognition of intangible factors is clearly evident. Thus, Hull now emphasizes that motivation is required for the overt use of habits. Spence now emphasizes the concept of secondary motivation (we might almost say, "goal expectations"). Douglas Lawrence is insisting that the concept of attending is, after all, a good S-R concept. Virginia Sheffield and Skinner are insisting that S-R theory can encompass the idea that, in intermittent-reinforcement situations, the "stimulus" is a temporally-extended pattern of action composed sometimes of hundreds of separate acts. Tracy Kendler is insisting that S-R psychology can assimilate the concept that discrimination behavior may be linked to a pattern, rather than to each stimulus-object separately.

The preceding paragraph brings me to my final point and, really, my most important point. I have been talking, through this paper, as though the main trends in personality theory have been a number of trends towards reliance on relatively more intangible factors. Now I would like to correct that statement, or make it more accurate.

You will remember that I defined "tangibility" as a matter of perceptibility of some variable. Now we need to focus on this fact: Perceptibility (or tangibility) is not some quality just of a variable as such. It is a property which exists as a function both of the thing itself and of the individual who is doing the perceiving. It is a function partly of the knowledge, skills, techniques, tools, and theoretical background of the perceiver. Consequently, as the work develops in any area, it has the consequence that it makes many factors "tangible" or "perceptible" which previously were "intangible," "elusive," and so on.

We need to realize that there are really a variety of ways by which tangibility can be increased. The first of these, I suppose, is just the basic process of getting an hypothesis that such and such a factor, or such and such a relationship, exists. When Otto Rank and Carl Rogers got the idea that there is an "urge to grow," which they believe has such and such effects, they were starting to create some tangibility for what possessed, before, only intangibility. Getting a name for a process, and particularly an apt name, such as Gordon Allport's "functional autonomy of motives," is a bit more of a contribution. Getting a clear, readily understood definition is a further step. Still more is done by providing some striking demonstration-material, even though it does nothing more than illustrate rather than prove. Thus, when the S-R psychologists demonstrate that a white rat can be trained so that it will strike an innocent rubber doll in a situation where the rat never before had experienced a rubber doll, they are not proving that displacement exists in human personality. But they are providing some of the intellectual background which makes the displacement process less intangible than it would have been otherwise. Still further, some real progress towards tangibility has been made when separate ideas and hypotheses are woven together into a coherent, logically well-developed total theory. Things are made more tangible when some methodology is developed whereby they can be studied. Things are made more tangible when some careful scientific evidence is gotten that such and such a factor must really be recognized and allowed for, if we are to make accurate predictions under some great diversity of conditions.

If we go back to some of the earlier theories of personality, such as Alfred Adler's, especially in some of his less commonly known concepts of social interest, individual goals, and life-style, we can find a great many factors which are tending to reappear in personality theory as it is currently developing. But, the thing we also can see is that Alfred Adler's system, for instance, lacked virtually every one of the sources of tangibility which I mentioned above, except the first few of recognizing certain variables, giving them names, and providing some bits of vivid illustrative material. There were no careful definitions, no development of a coherent logical system, no provision of research methodology, no gathering of convincing research material. So, the factors which Adler sensed remained intangible even though Adler felt that he should have been able to communicate his ideas to other persons.

In the theories of personality, then, there is an over-all trend which possesses great significance. It is a trend, in a sense, towards more objective and tough-minded principles—principles phrased in terms of increasingly tangible factors. But, the reason for this is not what some psychologists have imagined. It is not the fact that personality theories are being phrased, more and more, in terms of factors which were tangible already. Instead, the chief value of psychology lies in the fact that it has been creating tangibility for those factors which can be proved to be significant, but which had always been elusive and unmanageable before.

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LEARNING THEORIES

Harry F. Harlow

THE meaning of learning theory differs in individuals according to their interests and biases. Too much of learning theory of today has fallen victim to minusculization with the goal of demonstrating a mathematical relationship—any mathematical relationship—between two variables of dubious importance. For those whose interest in learning theory extends beyond scientific social approval, there are more challenging problems than the plotting of ubiquitous and ever-present curves of negative acceleration. The primary goal of learning theory should be integrative—to discover and demonstrate fundamental relationships between broad categories of behavior, if such relationships truly exist. If attempts of this kind do not represent current trends in learning theory today, it is at least to be hoped that they will represent current trends in learning theory tomorrow.

Present-day psychology is still described in such faculty-psychology categories as perception, learning, and thinking—categories that are useful and appropriate until the existence and nature of underlying and integrative mechanisms can be demonstrated. It is to the credit of the Gestalt psychologists that they essayed to integrate perception, learning, and thinking into a unitary theoretical structure. Unfortunately, their theoretical structure overemphasized the nativist position, assumed that perceptual processes could be reduced to natural gestalten of idealized Pythagorean perfection, and offered no mechanism other than an intuitive perceptual reorganization to account for behavioral changes. No possibility was provided for the factor of learning in the development of perception. For these theorists, rather, perception dominates and determines the nature and direction of learning.

The Neo-Behaviorists have ignored the problems involved in the learning of the perceptual process. There are good reasons why this is true. Not the least of these is the difficulty of analyzing these processes in the rat. Further, in the behavioristic response psychology, perception is of necessity a construct, a process inferred from the fact that different kinds of stimuli are discriminated with unequal facility. This systematization tends to de-emphasize the importance of perception and "perceptual learning," i.e., the influence of various categories of stimuli on rate and efficiency of learning. Yet no true quantification of learning, generalization, and transposition is possible until adequate scaling of the involved sensory-perceptual dimensions has been accomplished.

The primary purpose of this paper is the presentation of current trends in learning theory, more specifically, perceptual learning theory. Before this can be adequately presented, attention must be given to the nature of perception in animals ranging from naive to sophisticated. Some of the data we shall present primarily relate to stimulus factors directly basic to perception and, indirectly, therefore, to learning efficiency. We shall show that these factors can be reduced to quantitative stimulus properties whose differentiation is subject to learning. This is in contrast to the Gestalt position that perception is in large part based on the qualitative characteristics of the stimulus per se, and these characteristics determine perception and, indirectly, learning, regardless of experience factors.

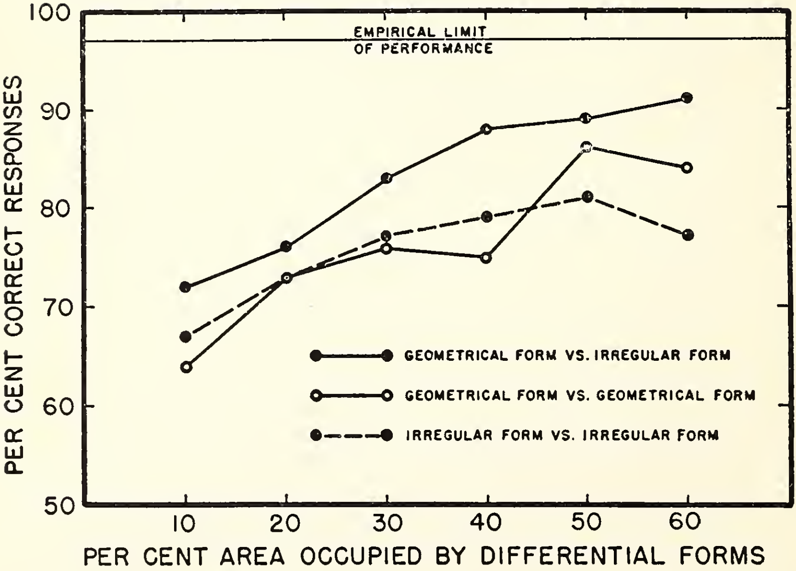
The first stimulus property we shall consider relates to the contribution of regularity or simplicity of form to ease of discrimination, a factor to which the Gestalt psychologists attribute great significance. The first experimental test of this stimulus characteristic on subhuman subjects was made by Lashley[11] in a series of exploratory studies using rats. No significant difference was found between discriminability of regular forms—a star against an H—and two ink-blot forms. Furthermore, no significant differences were found between speed of discrimination of two figures composed of lines arranged in a regular geometrical order, and two of Thorndike's "figures lacking identifiability."

Although these data indicate that no advantage was derived from the regularity of geometrical design, the generality of the conclusion is limited by a number of factors: the number of subjects was small, as was the number of tests; the variance in intratest performance was large and the statistical analysis limited. Finally, rats tend to respond to very limited portions of the visual field, a fact stressed by Lashley.

Further data on the relative ease of discriminability of geometrical as opposed to nongeometrical figures has been obtained by Warren, using seven rhesus monkeys as subjects. The stimuli were five geometrical and five nongeometrical forms cut from black paper and mounted on three-inch white cardboard squares.

The ten forms yielded forty-five comparisons, of which ten were between two geometrical forms, ten between two nongeometrical forms, and twenty-five between geometrical and nongeometrical forms. These forty-five paired comparisons were replicated under six size conditions in which the forms occupied 10, 20, 30, 40, 50, and 60 per cent, respectively, of the total area of the three-inch-square cards. Each subject was tested for fifteen trials on each of the 270 discriminations.

The data are presented in Figure 1. For the moment we ignore the variable of size. The differences between



DETAIL FORM DISCRIMINATION AS A FUNCTION OF THE NATURE OF THE STIMULI COMPARED. BACKGROUND CONSTANT = 9 SQ. INCHES.

Figure 1

the performance on the paired geometrical and the paired nongeometrical forms are not significant; the differences in performance on the paired geometrical and nongeometrical forms versus each of the other two conditions of pairing are significant at the 2 and the 1 per cent confidence levels, respectively. If ease of perception is a function of idealized geometrical form, as the Gestaltists imply, the paired geometrical forms should have been most easily differentiated, the paired nongeometrical forms least easily differentiated, and the mixed pairs should have occupied an intermediate position. The obtained data do not, of course, support this position. An additional factor argues against the concept that Euclidean-type forms are more natural and differentiable than nongeometric forms: the subjects had had extensive immediate past experience with a long series of geometric form discriminations. Other things being equal, this training should have favored the learning of the geometric stimuli.

It is an interesting and probably significant fact that no experiment on subhuman animals has yet given any indication that regular forms are more "natural" or "innate" than irregular forms. From the accounts of the visual experiences of individuals congenitally blind and later given sight by surgical operation, there is no indication that geometrical forms are better differentiated than irregular figures; indeed, there is no indication that the boundaries of geometrical figures are seen as sharply defined, smooth, and regular.

The neurological data on the recovery of vision following trauma of the occipital and preoccipital areas[18] show that during the process of recovery the patient comes serially to differentiate brightness, crude figure-ground relations, groupings and numbers, and subsequently—if at all—precise, regular, geometrical figures.

Similarly, Helson and Fehrer,[9] using normal human subjects, found three levels of perception. Their subjects first reported vague formless light, then a formed object which could not be correctly identified, and, finally, the correct form.

The Gestalt experiments in "Pregnance" clearly show that individuals having years of training in the perception of geometrical figures, perceive slightly imperfect or incomplete figures, tachistoscopically presented, as perfectly geometrical. The conclusion that geometric forms are innately "good" is, of course, completely without justification. The critical experiment would be to present to the subjects pictures of their friends' faces. If, tachistoscopically presented, these faces were to assume geometric forms, the Gestalt position would be immeasurably strengthened and, of equal importance, a rationale for cubistic art would be established.

Experimental investigations of the factors producing changes in forms reproduced from memory show that regularity or irregularity of the reproductions and dominance of linearity or curvilinearity in the drawing are obviously dependent on experience, including language experience.[2]

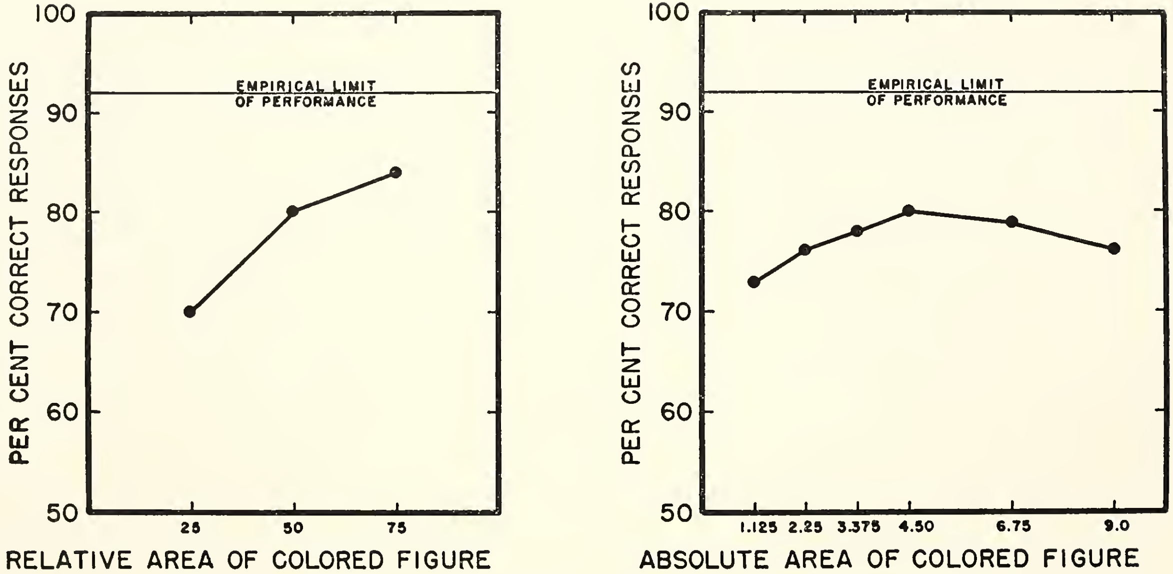
There is every reason to believe that form perception is a function not only dependent upon learning, but upon the learning imposed by limited cultural groups. The real, nonlaboratory world is primarily composed of nongeometrical forms, and the primitive man learns to effect perceptual differentiation within categories of these forms at a level that startles and bewilders the traveler and explorer, trained early and long in geometrical design. Animal forms, nests, trails, and landmarks are essentially irregular, and the wild beast seldom arranges its spoor in precise Pythagorean patterns.

Kind of contour or outline of form per se appears, thus, not to be a factor in discriminability. What, then, are the properties of form stimuli that do determine perceptual ease? Our interest in one of these properties —differential areal extent of forms in relation to the area of the ground—arose from a study we reported a number of years ago. In this particular experiment we uncovered a phenomenon so striking that we could hardly believe it, and either no one ever read it, or if anyone did, he could hardly believe it either! Monkeys presented with two objects such as two-inch cubes, one red and one blue, quickly learned to differentiate these stimuli. These same objects glued to the faces of identical white wedges each having a total face area of twelve square inches were, however, differentiated by the monkeys with very much more difficulty. Differences in performance on sets of problems, of which the one described is representative, were statistically significant. Similar results were obtained when two-dimensional, planometric stimuli were presented with and without the wedge background.

These data suggested that a functional relationship might be described between percentage of the differential area of two objects and the objects' discriminability. This hypothesis was tested on seven rhesus monkeys at the University of Wisconsin in the experiment described as relating to regularity of contour. It will be recalled that the various geometrical and nongeometrical figures varied in size so that they occupied from 10 to 60 per cent of the area of the constant-sized cards. We now consider the data relating to this size variable. Figure 1 again shows correct response plotted against figure area. It can be seen that efficiency of response increases exponentially as a function of increasing area of the cards allotted to the differential figures. These data clearly show that under the conditions studied, differentiability between stimuli is a function of the percentage of differing area on the otherwise identical stimuli. The two reversals of trend for the largest figures are doubtless occasioned by an artifact. Some of the figures were of such shape that their outlines could not be contained on the three-inch-square cards when the figures were increased in size to make their areas 50 and 60 per cent of the total stimulus area. Consequently, the figures were necessarily modified at these large sizes, thus changing somewhat one of the variables held constant for the other four size conditions.

To determine whether the increasing efficiency with increasing figure size might be attributed to the enlargement of the figures per se, rather than to the enlargement of the figures with respect to the backgrounds, a preliminary experiment was also conducted by Warren on a single, highly-trained monkey. Six sizes of differential pairs of figures, ranging from 1.125 to 9 square inches, were centered on pairs of white cards of varying area such that each size of figure appeared under conditions in which it occupied 25, 50, and 75 per cent of the total card size. No consistent relationship was found between absolute area of the figures and efficiency of response, whereas efficiency of response was found clearly to be related to the relative area the figure occupied. This is indicated in Figures 2a and 2b. The efficiency of the animals' response to the largest absolute and relative sizes is probably lowered by the fact that some stimulus plaques as large as six inches square had to be used, a size which is too large to be manipulated effectively in our standard apparatus and which frequently occasions avoidance responses by monkeys.

Further analysis of factors influencing discrimination ability was made by testing eight monkeys on a total of 450 problems run for ten trials each. Again the basic stimuli were three-inch by three-inch cards, and on these were pasted ten different configurations differing only in color for any particular discrimination. The figures occupied 100, 75, 50, and 25 per cent of the cards, respectively, and for each of the last three sizes, three different patterns were tested: (1) a colored border around a central white square, (2) a colored square centered on a white background, and (3) a vertical colored bar centered horizontally on the face of the card. Figure 3 shows discrimination efficiency as a function of the percentage of the card area differentially colored. Efficiency increases progressively with increasing size of colored area from 25 to 75 per cent but decreases slightly from 75 to 100 per cent differential area. Response to the 75 per cent differential border and the 75 per cent differential bar is superior to response to the 100 per cent differential area. It is

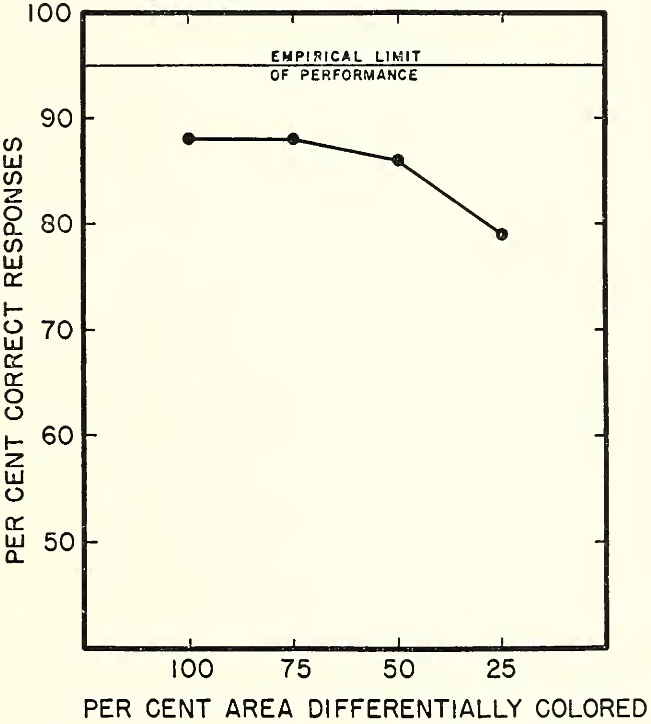


COLOR DETAIL DISCRIMINATION ON WHITE BACKGROUND
AREA OF BACKGROUND IS VARIABLE.

Figure 2a

Figure 2b

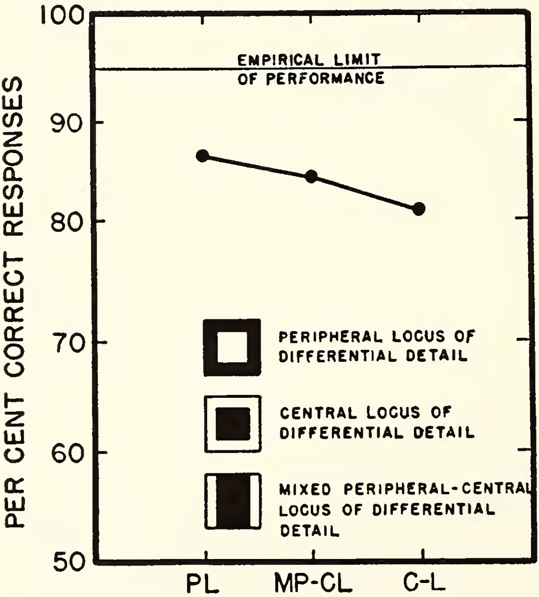
to be noted in Figure 4 that the cards with the colored borders were discriminated better than the cards with the vertical bars, and both were superior to the cards with the colored squares. These findings suggest that total perimetric extent of the differential areas—in the



DETAIL DISCRIMINATION AS A FUNCTION OF RELATIVE SIZE OF FIGURE. BACKGROUND CONSTANT = 9 SQ. INCHES.

Figure 3

case of the colored borders, combined outer and inner boundaries—operate as a factor influencing discriminability. This is logically in keeping with the fact that the perimeters provide the animal with gradients of visual excitation. If efficiency of response is plotted against the total perimetric extent of the colored area of the ten configurations, perfect correspondence is found except for the 50 and 25 per cent areas of colored



DETAIL DISCRIMINATION FOR COLOR DIFFERENCES AS A FUNCTION OF THE LOCUS OF THE DIFFERENTIATING COLOR.

Figure 4

border, which are less efficient than the 75 per cent area of colored border, and considerably less efficient than would have been predicted. The hypothesis that discriminability is a function of both perimetric extent and percentage of differential area may give a final solution to our problem. Further investigation is needed to test this possibility.

The results regarding the apparent significance of area and perimeter in discrimination of form, and the unimportance of regularity or simplicity of forms per se for the monkey, find interesting support in a recently published study by Casperson[3] on form discrimination in human subjects. This investigator used six basic solid geometric forms—an ellipse, diamond, triangle, rectangle, cross, and star—and presented five variations of each basic form. Correct recognition of the forms was the criterion of discrimination. The per cent correct reports by twenty subjects was plotted against area, maximum dimension, and perimeter. It was found that perimeter was the best predictor of discriminability for the total thirty forms, and for stars and crosses, in particular; that maximum dimensions predicted best for the six basic forms as a group, and for the rectangles and diamonds, in particular; and that area was the best measure for ellipses and triangles. In spite of differences in species and techniques, the comparability of general findings between the Wisconsin studies and the Casperson investigation is provocative.

It is obvious that we have no final answer to the nature of all factors basic to stimulus differentiability, but it is equally obvious that we have the basic techniques to go far in solving these problems—at least for the monkey. The basic nature of these factors is inferred from the fact that consistent quantitative differences are found in the responses of animals whose training records indicate that the possibility of further gain in discrimination learning has approached the vanishing point. Whether or not these factors are innate is an unsolved problem, for we used highly sophisticated animals. It would be necessary to devise a technique usable on newborn or visually deprived subjects if the nativistic-learned aspect of this problem is to be solved.

Entering the happy field of pure speculation, we would hazard that the only basic perceptual factors are size of differential brightness-hue areas in the perceptual field, steepness of gradients between the parts of the visual fields containing the differential areas, and total boundary extents. Such an hypothesis implies even less emphasis on the role of nativistic factors and even greater emphasis on the role of learning in visual perception than the recent similar thesis advanced by Hebb. Hebb[8] compromises with Gestalt psychology to the extent that, "Lines and angles, then, can be treated as perceptual elements, not fully innate in perception, but partly so, and likely to be learned before more complex patterns are. . . . If line and angle are the bricks from which form perceptions are built, the primitive unity of the figure might be regarded as mortar, and the eye movement as the hand of the builder."

Since there is no evidence in the Warren study that nongeometrical figures, including figures with curves and curlicues, are differentiated with more difficulty than more linear and more angular geometric figures, there is no reason to emphasize "lines and angles." It is true that Helson and Fehrer,[9] Whitmer,[20] and other workers have found that the circle ranks after the rectangle and triangle in perceptibility, but if lines and angles have any particular role, it may well be a function of simpler principles such as those we have already suggested; e.g., for any particular area a circle offers minimum perimeter, a square more, and a triangle still more. A square has less perimetric extent than two parallelograms of equal total area.

Hebb's emphasis on lines and corners is influenced by the continual emphasis he places on motor activity. He states:

> . . . there is a strong tendency for the eye to make a sweep along the line in one direction or the other; and, as the sweep is made, at the moment a corner is reached the stimulation of the intersecting line is at a maximum, since at this moment every point in the second line has the same vector, for a new direction of movement.[8]

Hebb offers no experimental support for this statement, and it is the speaker's belief that it is not supported by fact—the eye movements of individuals surveying symmetrical, continuous linear, or curvilinear forms are irregular and in no wise conform to the geometry of the stimulus (see 8, p. 34, par. 3). Hebb emphasizes the counting of corners by Senden's congenital cataract patients, but there is little indication of any permanent gain by the patients who so behaved. Such behavior would be an expected transfer by a person dependent for years upon cutaneous kinesthetic cues and highly sophisticated in language symbols associated with such cues. Citing the observation of Kennard[10] and others that unilateral lesions in Area 8 produce transient homonymous hemianopia, Hebb suggests that " . . . perception intimately involves a motor activity, liminal or subliminal . . . it must have something to do with the elaboration of sensation into visual perception."[8]

The assumption that kinesthetic cues, manual or oculomotor, have any functions in visual perception other than those of spatial orientation and essential fixation, rests on entirely tenuous grounds. This behavioristic anachronism has led various authorities to explain why animals can differentiate stereometric objects more effectively than planometric objects. If the eye movements played an essential role in vision, the hypothesis should be supported in fact. Actual tests made on monkeys by Harlow[5] revealed, however, no significant difference in the ability of monkeys to differentiate stereometric objects and planometric objects of equal face area, in spite of the fact that the total area (i.e., face and sides) was greater for the stereometric stimuli. Unless supported by direct experimental evidence it is to be hoped that muscle-bound theories of visual perception will be put to rest and allowed to atrophy as peacefully as possible.

Attention has been given to the relative importance of form and of color in perception. Thus Senden reported patients' segregation independent of experience of a patch of color with ill-defined boundaries, and he emphasized that color dominated form persistently in the first vision of these patients. Surface color, however, does not exist independently of form. The problem is not that of dominance of color over form, but rather, either of two problems. One problem relates to the difference in discriminability of forms varying in hue as well as shape, and forms varying only in shape. The other problem refers to the relative discriminability of identical forms differing in hue versus different forms identical in hue. There is a wealth of data to indicate that the discriminability of two objects differing in form is greatly enhanced if they also differ in hue. There can be little question that hue differences alone offer more effective cues than form differences alone to human beings after operative recovery from congenital blindness,[8] to normal chimpanzees,[15] and to normal monkeys.[14] Unfortunately, the data for normal human beings are indeterminate (cf. 1, 22).

It is possible by constantly testing monkeys to train them on discrimination tests in a few years to the point that their learning curves on any kind of stimulus category or combination of stimulus categories approach asymptotic functions. Thus, we can hold form and color constant and determine relative discriminability of any pair of size values, or we can hold size constant and determine relative discriminability of any or all categories of form or color, or we can measure the effect of combining any set of attributes which we wish. Within the limits of the validity of such direct comparisons we are in a position to scale quantitatively and compare directly any and all visual functions, limited only by availability of monkeys, time, laboratory space, and financial support. Within such limits we can even investigate the properties of the gaunt, gray ghost of "afferent interaction"—the missing link in rodentological learning theory.

So far, we have presented data and dreams bearing on the fundamental nature of perception, and techniques for further attack on the problem. We have hypothesized that there are few, if any, visual properties that are not greatly modified by learning, and that learning is the key both to understanding perception and to integrating this psychological faculty with other time- and tradition-honored faculties in psychology.

In his recent book Hebb[8] has forcefully and brilliantly presented the position of the role of learning in perception, particularly visual perception. He has appropriately cited the observations of Senden, which stress the limitations in perception following successful operation for congenital cataract and the great difficulty these patients have in learning visual discriminations and associated word names, months and even years after operative recovery.

> Senden reports cases in which there was an immediate perception of differences in two figures seen together, but also a number of instances in which even this was not possible. Thus a patient sometimes saw differences between a sphere and a cube, sometimes not. Color has been found to dominate form persistently in the first vision of these patients. Eleven months after operation the color names learned by a patient in hospital were retained, but the little that had been learned of form was forgotten. An egg, potato, and cube of sugar were seen by a patient repeatedly, until naming was prompt, but then were not recognized when put into colored light; the cube of sugar was well named when it was seen on the table or in the investigator's hand

but not recognized when suspended by a thread with a change of background.[8]

A patient was trained to discriminate square from triangle over a period of 13 days, and had learned so little in this time "that he could not report their form without counting corners one after another. . . . And yet it seemed that the recognition process was beginning already to be automatic, so that some day the judgement 'square' would be given with simple vision, which would then easily lead to the belief that form was always simultaneously given."[8]

Equally effective evidence that the initial visual discriminations of primates are *very slowly learned* is found in the brilliant studies of Riesen,[19] who reared chimpanzees and monkeys in darkness for periods of as long as two years. Two chimpanzees, Alfalfa and Snark, were kept in a completely darkened room from birth to sixteen months of age and on a limited light schedule from that age to twenty-one and thirty-three months of age, respectively. Normal recognition of objects, blinking to threats, and the cessation of startle to touch developed gradually over a period of many months in Alfalfa after she was moved into daylight surroundings. Snark learned over the course of many hundreds of trials to discriminate large signs differing in color or pattern and indicating either food or mild electric shock.

Chimpanzee Debi was raised for seven months in complete darkness; Kora was raised for the same period on a schedule which provided an average of 1.5 hours of light daily through a white Plexiglas mask admitting only homogeneous, unpatterned light; Lad was given 1.5 hours of patterned light daily. Lad rapidly learned normal visual responses. Whereas normal animals of seven months learned to avoid a large yellow-and-black-striped disc after receiving one or two mild electric shocks from it, Debi and Kora were shocked from it twice a day for six and nine days, respectively, before they so much as whimpered when it was shown them preceding shock. It was thirteen days for Kora and fifteen days for Debi before they consistently showed some avoidance response within five seconds of the time that the disc was raised in front of their eyes.

The detailed observations of Senden and the beautifully conceived and executed experiments of Riesen attest to the fact that the original learning of visual materials is extremely slow and imperfect almost beyond belief.

The extant data clearly indicate that a tremendous gulf exists between the original learning of visual perceptions and the capacity of visual differentiation that characterizes the adult primate. The simplest visual discriminations are learned slowly and painfully, even if motivated by shock, in the naive primate; the sophisticated primate learns many, if not most, visual discriminations immediately or in a single trial; in fact, if he is a human primate, he becomes so proficient that he loses all insight into how his visual perception developed and substitutes, instead, a theory of the same name.

Analysis of the mechanism which operates to transform the slow visual learner into a rapid visual learner has been conducted at Wisconsin during the last two years. It will surprise few of you to learn that we believe this mechanism to be the formation of perceptual learning sets. Since learning set theory has been treated extensively elsewhere,[6] we shall only very briefly describe it here.

Learning set theory was developed by studying the learning by normal monkeys of long series of discrimi-

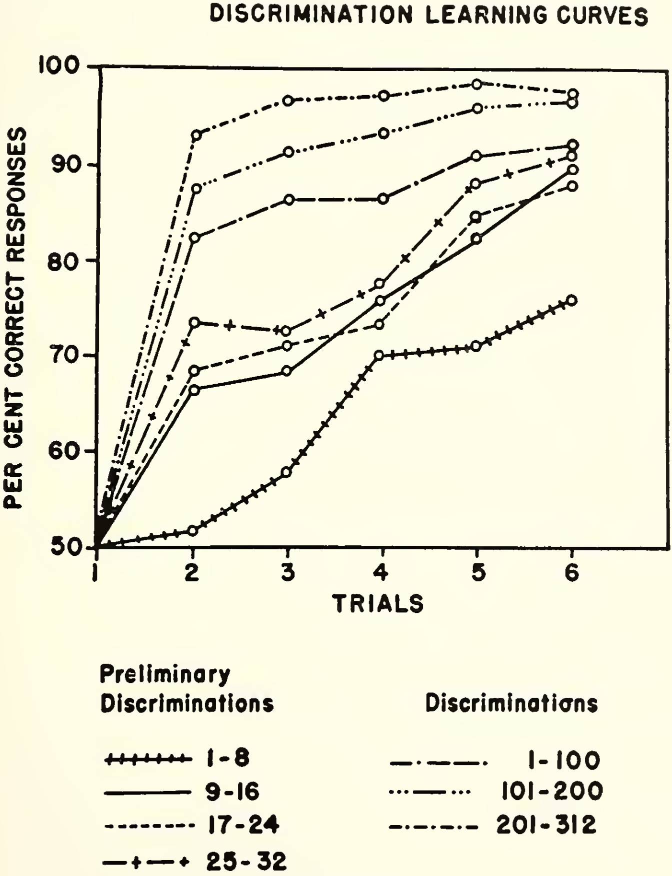


Figure 5

nation problems using stimuli which were largely stereometric and which differed in multiple dimensions. The data of Figure 5 show that the early problems were learned by slow trial-and-error processes, but that interproblem learning converted this painfully slow and ineffective acquisition process into one which was amazingly rapid and proficient. By the time the animals had learned 300 six-trial multidimensional stimulus discrimination problems, they were averaging 97 per cent correct responses on trial two—the direct measure of the performance gain resulting from response on trial one.[6] And, no doubt, by the time the monkeys had attained this level of proficiency, they believed that the ability to perceive differences in multidimensional stimuli was innate, that their quick insight resulted from perceptual reorganization of the spatial whole, and that the ability to solve problems of such complexity by reasoning set monkeys apart from all other members in the phyletic scale.

Monkeys also acquire learning sets to solve series of multidimensional, planometric discriminations whose stimuli are fabricated from three-inch squares cut from magazines, newspapers, linoleum, wallpaper samples, and the like. All nine monkeys tested on a series of 216 such problems formed highly efficient learning sets.

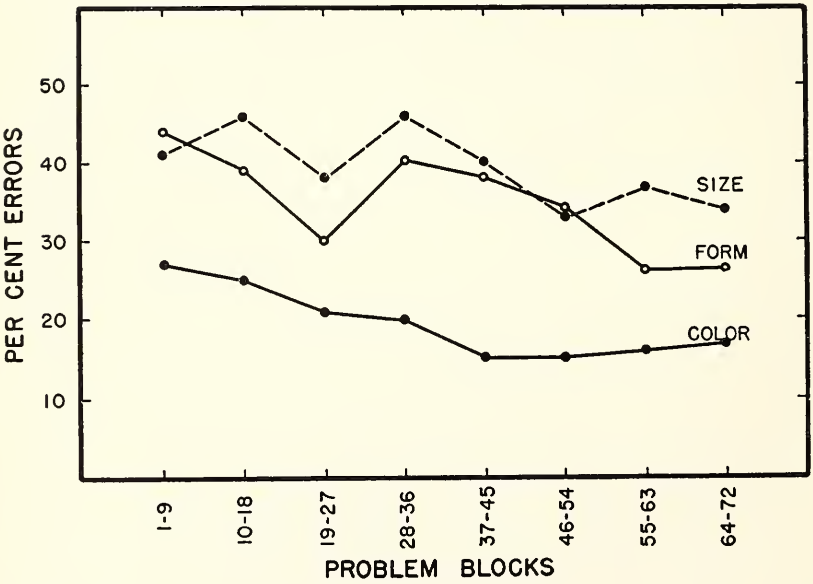
These data do not, of course, differentiate between learning on the part of the animals to perceive differentiating characteristics of the stimuli—assuming of course that these were learned—and the learning of the response tendencies essential for the mastery of all nonspatial discriminations. Detailed analysis of the basic factors underlying these response tendencies has already been published in *Experimental Psychology.*[7]

A group of four rhesus monkeys tested continuously for four years on hundreds of problems, including 280 discrimination problems, was subsequently tested for the formation of learning sets for *specifically defined perceptual categories.* The stimuli were white cards three inches square with construction paper figures pasted upon them. These stimuli were divided into six categories—stimuli differing in color and form, color only, form only, number (1 vs. 2), size (1:2), and detail (limited to stimuli with a maximum dimension of 0.5 inches). Thirty-six problems were run on each category, and each problem was tested for fifteen trials. The problems were presented in random order with regard to categories.

To our surprise little evidence was obtained for the formation of perceptural learning sets using these difficult pattern stimuli. Mean performance never surpassed 65 per cent correct, error scores dropped only 8 per cent from problems 1 to 36 to problems 181 to 216, and performance in only two categories, color and form, and color, was significantly better than chance. In no category did performance exceed 75 per cent correct.

A year later the same four monkeys were tested with similar stimuli from three categories with color, form, and 100 per cent size difference. The data are presented in Figure 6 and give evidence for perceptual learning set formation in each category. The differences between problems 1 to 12 and problems 36 to 48 in each category are significant at the 1 per cent confidence level, and the absolute gains for color, form, and size are nearly 10, 16, and 8 percentage points, respectively. These data show that learning sets can be formed that enable monkeys to respond with increasing efficiency to specific perceptual categories. At the same time, the data strongly indicate that such perceptual learning sets would never reach perfection in the rhesus monkey, at least not with such techniques as we presently employ.

The combined data of the two experiments just cited suggest that considerable interference may result from



PERFORMANCE OF NORMAL MONKEYS ON PATTERN DISCRIMINATION PROBLEMS. STIMULI VARY IN FORM, SIZE OR COLOR.

Figure 6

attempts to form perceptual sets for many different categories simultaneously. Data from many different experiments on monkeys demonstrate that color perception learning sets are formed more rapidly than form-perception sets or size-perception sets (defining size as 100 per cent areal differences between stimuli).

Whatever limitations the monkey may have in learning quickly to differentiate stimuli belonging to various categories—and we are the first to insist that these limitations are tremendous if we compare monkey with man—the monkey is truly a perceptual giant compared with other subhuman animals.

No one has yet demonstrated that the rat possesses the ability to form effective perceptual sets enabling it quickly, let alone immediately, to differentiate between stimuli belonging to a single perceptual category. Recently, Lawrence[12, 13] reported that rats may acquire distinctiveness of cues, but the acquired distinctiveness so far demonstrated is distinctiveness of single, isolated, individual stimuli, and not distinctiveness among all stimuli lying within some broad perceptual category. North[16, 17] has demonstrated that rats may show improvement in the reversal of successive black-white discriminations, and properly states that a possible explanation of this phenomenon *may* be made in terms of distinctiveness of cues.

The "concept formation" studies initiated by Fields[4] using rats as subjects, and subsequently extended by others to cats, dogs, monkeys, chimpanzees, and children, attest to varying—but limited—abilities of these subjects to form even a restricted type of perceptual learning set.

If we measure the ability of animals—particularly monkeys—to form various perceptual learning sets, and find that the efficiency of such set formation differs for varying perceptual categories, we cannot assume that this proves that the varying difficulty of different perceptual categories in turn influences the efficiency with which these sets can be used in the solution of learning problems. Fortunately, we are in a position in which we are not forced to hypothesize about this relationship. For the monkey, at least, we have a wealth of data showing a high degree of consistency between the difficulty of acquisition of varying perceptual learning sets—as measured by discrimination tests—and the effectiveness with which such materials can be employed in solving problems of greater instrumental complexity.

In deference to man we would suggest that the ability of the human animal to form and utilize multiple perceptual learning sets probably differentiates him from other animals to as great a degree as does the ability to utilize spoken language. Even monkeys and chimpanzees show very limited ability to form effective sets to differentiate tiny, detailed stimuli, or tiny, detailed variations between stimuli, in contrast to the human beings whose incredible skill along these lines enables him to read the *Scientific American, Time,* the *Reader's Digest,* the *Daily Worker,* and the *Chicago Tribune.* Man is defined as the speaking animal, but of equal importance is the fact that he is also the only animal capable of forming the detailed and differentiated perceptual learning sets that underlie a vast range of intellectual processes, including those of mastering or being mastered by the printed page.

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THEORIES OF PERCEPTION

James J. Gibson

THE aim of this conference, I suppose, is to make a sort of inventory of the intellectual tools available for the advancing of knowledge in psychology. In the case of perception, the inventory will not be easy. The stockroom is crammed with tools, but there is no good system for putting them in order. Some are dull and outworn. Some appear to be makeshifts—the favorite device of one mechanic but scorned by everybody else in the shop. Undoubtedly, the place needs new equipment and the workers ought to get together about the problem.

A survey of the theories of perception might start with an attempt to classify them in more or less traditional ways. There are the categories of nativism and empiricism, structuralism, functionalism, and configurationism, phenomenological, psychophysical, field-theoretical, physiological, and so on. These terms provide a fairly convenient set of theory-bins. But classifying the theories does not, in itself, do anything to sharpen the tools. The bins are not very appropriate for the uses to which the theories must be put. Furthermore, the classifying of theories leads to the labelling of investigators, and this is apt to be uncomfortable and often unfair.

A more useful beginning would be a survey of the various fields of inquiry which come under the heading of perception. The term perception, as we shall see, means different things to different psychologists. The aim should be to get an over-all view of the different meanings of the term, the kinds of interest now being shown in the subject, and the corresponding methods of investigation being employed. There are, I suggest, about four such types: (1) an interest in perception considered as discrimination, (2) an interest in the perception of objects and events in space, (3) an interest in the perception of the meanings and values of things, including people and symbols, and (4) a widespread interest in the sort of perceptual game which can be induced by looking at inkblots, and which is characteristic of the individual perceiver.

PERCEPTION AS DISCRIMINATION

The ability of men to discriminate the variable of physical energy applied to their sense organs has been studied for a long time. The method employed is the psychophysical experiment. The theory and practice of this method have been developed further, perhaps, than is the case for any other experiment in psychology.[2] A subject can easily be led, by instruction or by training, to give judgments or differential reactions to isolated variables of stimulation, and the procedure does not appear to be fundamentally different in animals or children from what it is in adults. It is essential for this experiment that the stimulation be carefully determined by the experimenter and that the conditions for observation be optimal. The resulting body of facts is perhaps the most securely established we have. It is called psychophysics or, with the addition of some other techniques, psychophysiology. It has given birth during recent years to a new branch of applied psychology with the ambitious name of human engineering. Human engineering is very much concerned with the discriminative capacities of man. The subject is flourishing. Already there has appeared a textbook of psychology for engineering students[6] and a handbook of facts, mostly psychophysical ones, for practicing design engineers.[12] There are laboratories of psychology devoted almost entirely to this type of research—at Harvard, Hopkins, and Tufts, for instance.

The theory of perception which goes with this field of interest usually assumes that experience is constituted by a discoverable set of variable properties, sometimes called attributes or dimensions.[2] The important term in this postulate is the word variable, for a variable is amenable to measurement, and measurement is one of the aims, if not the main criterion, of science. The students of modern psychophysics are sometimes accused of being sensationalists and elementarists[4] but this is hardly fair, for they know that the classical distinction between sensation and perception has broken down, and they do not mean by a variable of experience anything like an elementary unit of sensation.

It is true that the workers in this branch of endeavor, being busy and prosperous, have been slow to apply their theory and method to the kinds of perception which concern social and clinical psychologists. There are exceptions. Volkmann,[21] among a few, has used the theory of scales of judgment to make hypotheses about the formation of stereotypes and the level of aspiration. There is nothing in the theory as such which makes it inapplicable to such problems as perceiving the traits of a personality or the quality of a social act. But the application has not yet been made.

THE PERCEPTION OF OBJECTS AND EVENTS

There are psychologists who say that it is a useful exercise, before plunging into theories and experiments about the perceptual process, to examine how it feels to perceive. These phenomenologists, so called, do not object to the proposition that experience is constituted by variable properties, but they point out that the experience itself is of a *world*. This phenomenal world contains objects and events extended in space and time. These also need explanation.

This field of study is not particularly popular in this country. No large laboratories are devoted to it and not many investigators habitually employ the useful exercise of looking at the world. A few, in fact, militantly object to it in the interests of strict and consistent operationalism. These latter will make very slow progress, I suggest, in solving the real and practical problem of space perception.

The perception of a three-dimensional world has never really been explained in a satisfactory way, and the perception of the flow of events in this world even less so. The fact was made evident by the failure of psychologists during the last war to make any significant recommendations about the practical problem of how a man lands an airplane. The solution to the space-time problem is probably basic to any understanding of how we apprehend meaningful objects, values, and social events. Perhaps one reason for the chaotic state of perceptual theories outside of psychophysics is that in the eighty-five years since Helmholtz there has been so little advance towards the solution of the space-time problem.

Fundamentally all we have to go on, even today, is the theory of Helmholtz. This is the notion that we *learn* to see the two-dimensional images on the retinas as a three-dimensional scene. We do so by a process of unconscious inference or, in the most recent terminology, by making assumptions about the clues to be found in the images.[15] There are a whole host of objections to this roundabout and implausible explanation of depth, brilliant as its formulation was. The objectors, however, from Hering to Koffka, have made essentially negative arguments. They have not proposed a more plausible theory to put in its place. Helmholtz wins, but only, I think, by default.

The foundation of this theory of cues has been cracked, to be sure, by Koffka.[14] The more accurate term is "clues," for this is the word which honestly accepts the full implications of the theory. We are supposed to have within us a mechanism which functions like Sherlock Holmes. The sense data are cryptic and it requires detection to see what the world is really like. He proposed more plausible explanations of several features of the visual world which were quite different from the explanation by mental interpretation. Examples are the figure-ground phenomenon, the constancy of surface colors, and the constancy of size and shape. But a convincing account of depth and distance, in my opinion, never emerged from the Gestalt theory of sensory organization. Koffka himself was forced to refer to the cues for depth when he talked about three-dimensional vision. The outcome has been that current investigations of space perception find themselves in theoretical confusion. Sometimes they follow Koffka and sometimes they follow Helmholtz. They employ the theory of sensory organization for some purposes and the terminology of cues for others, although the roots of the two are contradictory.

This situation is what I have recently tried to remedy by a theory of stimulus-gradients.[10] Considering space as an array of surfaces and edges, there is reason to believe (despite the long-established contrary opinion) that the third dimension is not "lost" when the world is projected as an image but, on the contrary, is present in the retinal images as specific high-order variables of stimulation. Not only the adjacent order of energies on the retina—the so-called distribution—but also the sequence of energies may be treated as a stimulus if only we give up the assumption that a stimulus is to be defined as the application of energy to a single receptor-cell. Instead we must define stimulation as the *order* of energy applications to an array of receptor-units and assume that the order retains its identity when transposed over the array. If this definition holds true, a psychophysics of space perception is possible. The validity of the theory rests, of course, on the outcome of experiments which isolate the gradients and look for the spatial impressions which are supposed to correspond to the gradients. Such experiments have only begun to come out, but the first appear to be successful.[9] The theory assumes that whenever stimulation is determinate, spatial qualities are correspondingly determined. When stimulation is curtailed or impoverished, as it often is in the laboratory experiments with which we are most familiar, spatial qualities are correspondingly fluid. Only to the extent that stimulus-determination fails do we need to assume that the influence of attitude, expectation, assumption, or past experience is exerted.

As a corollary of this theory of space perception an explanation of the size and shape constancy of objects is suggested which likewise does not rely on the appeal to past experience or learning. The explanation is, following Koffka,[14] that whenever there exist reciprocal modes of stimulation (such as the stimuli for size and distance, and those for slant and shape) an *invariant* is present and that this, being constant, is simply the adequate stimulus for a constant object in perception.

There are signs that the interest in spatial and temporal aspects of perception is increasing. If an experimental method can be devised for the manipulation and control of these aspects, genuine discoveries are sure to be made.

THE PERCEPTION OF MEANINGS AND VALUES

The world we perceive is not only a world of surfaces, edges, and interspaces; it is a world of useful and dangerous objects, of signs and pictures, of people and groups, of social actions and events. All these are enormously variable but they fall into a finite number of classes, and after we have come to know a sufficient number of *typical* objects and events, we are able to deal with them efficiently. The third type of inquiry into perception is concerned with meanings and values in this sense. Although the perception of words and the interpretation of facial expressions are old problems, other problems in this field had been barely touched by experimenters until, under the influence of social psychology during recent years, interest in it has grown to large proportions.

Classical experimental psychology has always included a slow trickle of research on concept-formation and thinking, but this evidence has seemed as dry as dust to most social psychologists. Much more interesting and relevant to them have been a number of studies explicitly concerned with group phenomena in perception and cognition. An early example is the opinion of Walter Lippmann that men's ideas about the political world were what he called "stereotypes."[17] Public opinion consisted of such stereotyped ideas. Bartlett investigated the changes which a story undergoes in being retold repeatedly; he also studied changes in the memory of a visual form.[1] He concluded that a story, or a form, was altered in accordance with a "schema," and the evidence suggested that the schema was characteristic not only of the individual, but also of his culture. Interest was especially quickened by an experiment of Sherif who, with considerable ingenuity, set up conditions in which the members of a group mutually influenced each other's judgments and appeared to reach a consensus of perception which Sherif called a "social norm."[19]

Considering all the accumulated evidence of this sort, social psychologists began to have a much more ambitious conception of their subject. Social perception was not merely the top layer, as it were, of a man's awareness, but extended downward to its base. Perhaps *all* perception was social perception. Likewise all behavior was social behavior. Social psychology was not something to be studied after learning about individual psychology, but something to be considered as a basis for the discipline. This revolutionary conception called for a new program of experiments, and the program has been enthusiastically begun. The best known work is that of Bruner and his collaborators, recently summarized by Bruner and Postman.[4] A parallel is the theorizing of Cantril and his students, which takes off from a set of three-dimensional illusions devised by Ames.[5, 15]

The Bruner experiments have mainly been concerned to prove that the basic organization of a perception is determined by attitudes, motives, fears, interests, and values in the perceiver which are social in origin. The most dramatic of these was a result which purported to show that size perception could be determined by economic-class attitudes.[3] The method employed is generally that of measuring the constant error of a set of judgments under two conditions—one with lesser and one with greater motivation. The experimental setup is one in which either the physical stimulation is impoverished or the pattern is ambiguous, or at least conditions are not optimal for accurate judgments. A favorite device for curtailing stimulation is the tachistoscope.

The theories of what is now being called social perception are somewhat disjointed. One theoretical component is the notion of sensory organization, or the structuring and restructuring of a perception. This notion is employed in its descriptive sense rather than in its physiological sense of a brain-process, and it lacks explanatory power. Another more important component is Lewin's theory that a value, a factor of motivation, or what he called a valence is inseparable from a percept. Lewin argued that a man's perceptions motivated the man in and of themselves. This emphasis makes perception a functional or dynamic process and allies it to social conduct. Still other theoretical components are borrowed from many sources. The most general concept is attitude, which means all things to all men. Purpose and assumption are taken from Ames.[15] Inference, conclusion, or interpretation come from Helmholtz.[11] Expectancy and sign-learning are taken from Tolman.[20] Framework and social norm are from Sherif.[19] Sentiment comes from McDougall.[18] Conceiving, understanding, knowing, believing, judging, imagining are terms from the vernacular. However varied the terminology, a central conviction is shared by these students of social perception: they are sure that the doctrine of the passive perceiver who simply mirrors the world is a myth and is now disposed of for good. What a man perceives, they say, depends on his personality and his culture. Men of different cultures perceive quite different worlds. There are, in short, folkways of perception.

An enthusiasm for social psychology and a sense of the urgency of its problems is something that I share. But I cannot agree with the social perceptionists that the kind of problems they study are the prototype of all problems in perception. I would be willing to agree with them that all perception is in a certain sense socialized perception if they were willing to agree in turn that all perception is just as truly psychophysical perception. But they are (or some of them have been) inclined to deny the latter. They are tempted to conclude that their results invalidate the data of psychophysics and that even its basic method comes under suspicion. Such a conclusion could only come from a misunderstanding of their own method. Their experiments are designed to maximize the noncorrespondence of perception to stimulation whereas the psychophysical experiment is designed to maximize the correspondence of perception to stimulation.

There are two possible lines of effort, I suggest, in the study of social perception: the attempt to understand misperception and the attempt to understand perception. Only the first is being pursued with any degree of energy. We know that shape, size, color, and space can be altered as a function of attitude or motivation if the stimulus presentation is balanced just right to obtain this effect. We know that patterns, pictures, and symbols may be conceptualized, stereotyped, and distorted by perceptual customs, although there is little agreement as to the explanation of this fact. What we do not sufficiently understand is, for instance, how a face is perceived as such, or is perceived as the face of an acquaintance, or is perceived as the face of a hostile man. How do we perceive, for instance, that one person is being kind to another, bearing in mind that we do this with some accuracy? How do we perceive the intentions and abilities of a political candidate, taking it for granted that he does not fool *all* of us all the time? In other words, what do we discriminate and identify in these complex stimulus-situations which, when conditions are favorable, yields a correct perception? This ought to be the primary line of inquiry, but instead it is almost completely neglected. It is, of course, extremely difficult to isolate the complex variables in situations of this sort and it would be laborious to control them experimentally. But if we ever expect to understand these physiognomic and dynamic properties of perception, we shall have to do it the hard way—by setting up the conditions which produce them in an observer, using a procedure which is fundamentally that of psychophysical experiment.

THE RESULTS OF AMBIGUOUS OR EQUIVOCAL STIMULATION

The largest of all the current fields of interest in perception, measured by man-hours of effort, is concerned with the so-called projective tests. When a man looks at an inkblot, for example, he is said to "perceive" any of a variety of objects, people, animals, or events. Perceiving is not the right term for it, but neither is the term imagining, since the blot plays a part in inducing the experience. An attitude must be adopted by the subject somewhat similar to the attitude we adopt in looking at a picture or representation. There is an element of searching in it, and the situation must be taken as a task or a game. If a subject reported simply that he saw an inkblot, the psychologist would be completely frustrated. The impressions aroused are said to be "projective," that is, to have apparent external reference, but actually to be of internal origin. This term also is imprecise, since only a hallucination is fully projective in this meaning. The situation might be called a game of controlled misperception.

The Rorschach inkblots contain gradients of texture, shading, and color, including moderate gradients of varying definition, and steep or sharp gradients, the latter tending to produce margins. In this respect it is like an ordinary photograph, but the essential characteristic of the inkblots is that these are *not* the specific gradients and margins which yield the surfaces and edges of objects in a photograph. The edges and surfaces which emerge in experience are ambiguous or equivocal. There are stimuli for corners, curves, indentations, protuberances, and interspaces in the inkblots, but these stimuli are mutually discrepant or conflicting. Since the spatial properties are not consistent, the objects are not consistent and what is seen can be almost anything. The orthodox description of the inkblots is to say that they are "unstructured," borrowing a term from Gestalt theory, but this is a poor word since it has never been clear whether the physical stimulation lacks structure or whether it is only the perception which is unstructured or, for that matter, exactly what the term structure means.

If the terminology of Rorschach testing is inexact, the theory is nothing less than chaotic. In order to understand what the subject sees and reports, we should have to have a theory of visual meaning, imagination, or fantasy. What the subject sees has proved to be, in some degree, diagnostic of his personality. But more important than what he sees has proved to be the *determinants* of what he sees—whether the things he reports are constructed out of the variables of shading, or of texture, or of color, or of form. His relative sensitivity to these components of everyday perception is seemingly even more deeply diagnostic of his personality than is the content of what he reports. For this paradox there is no theoretical explanation whatever. The principle has been suggested that "perceptual organization is the key to personality organization,"[4] but this statement is scarcely more than a play on words. In a sense, the viewing of Rorschach cards seems to be a strange and peculiar form of space perception. If this is true, a theory of space perception is required before the Rorschach riddle can be solved. We must analyze space into the basic phenomenal variables of which it is composed—surfaces, colors, edges, shapes, and their combinations—and we must discover the gradients and steps of stimulation which regularly elicit them if we expect to understand individual differences and personal predilections in this kind of human activity.

Clinical psychologists tend to accept the generalization that perception is a creative process which constructs the world to suit the perceiver—that we see things not as *they* are but as *we* are. In this respect they follow the social psychologists except that they emphasize the bias of personality instead of the bias of culture. The danger consists of being led into a kind of undisciplined philosophizing about perception which can only distract them from scientific pursuits.

THE DISTINCTION BETWEEN LITERAL AND SCHEMATIC OBSERVATION

The diversity of perceptual theorizing and the difficulties in the way of consistent thinking may be illustrated by a table. It consists of all the nouns which might be included under the category of perception in the present broad usage of that word. The columns fall roughly under the neat and simple terms with which psychology was endowed by the structuralists: sensation, perception, apperception, and imagination. Our terminological trouble comes from the fact that we have rejected the distinctions between these terms but have not invented better ones to substitute for them. There is surely a difference between the terms on the left and those towards the right, but what is it—degree of meaning? Degree of organization? Or simply degree of independence from stimulation? How does motivation fit into the scheme, or attention, or learning, or ego-involvement? In the face of this difficulty, we have lifted the term perception from the middle of the array and made it stand for the whole set. As a result, the word is being made to bear the weight of too many different meanings. We need new distinctions, but where shall the lines be drawn?

Sensation	Perception	Apperception		Imagination
Sense data	Object perception	Recognition	Cognition	Fantasy
Sense-impression	Space perception	Identification	Apprehension	Projective perception
Stimulus-registration	Time perception		Comprehension	
Psychophysical discrimination			Ideation	
			Conception	
			Preperception	
		Value	Belief	
		Valence	Judgment	

The central issue in contemporary theories of perception, it seems to me, is whether to emphasize one or another of two contradictory propositions, the first that perception depends on stimulation, and the second that perception does not depend on stimulation but on something else. This "something" is variously described as a process of organization, or set, or motivation, or attention, or learning. Psychologists who prefer the first proposition emphasize psychophysical correspondence. Those who prefer the second proposition emphasize psychophysical noncorrespondence. An example of the latter emphasis in its most radical form is the set of demonstrations produced by Ames and Cantril[15] which aims to prove that even spatial impressions cannot be accounted for by the external objects and the optical stimulation, but must be explained by taking into account the purposes of the perceiver.

Whatever else may be said about the perceptual process this much is certain: it has to do with man's relation to the environment. We generally assume, and teach our students, that the relation is mediated by stimulation. It is true that there have always been some philosophers and psychologists who have argued that stimulation represents the environment so inaccurately that it cannot mediate this relation. The retinal image, for instance, is two dimensional whereas experience is three dimensional. The image is considered much too poor a thing to account for the richness of vision. If such be the case, the perceiver has to get his knowledge of the environment in some way from within himself. The search for this inner source has a long history. One possibility was that he had innate ideas of the world implanted in his mind. Another was that he derived his ideas from past experience, or by making and testing inferences about the world. A possibility recently suggested is that the structure of the brain process which yields his percept is like the structure of the external object by virtue of having the same laws of organization.[13] The simplest possibility of all, however, would be to assume that stimulation is not as feeble or inaccurate as it has been taken to be, and that it does mediate man's relation to his environment after all. This possibility gains weight if the clues for depth turn out to be formulable as gradients of stimulation, and if these prove to yield the qualities of space in psychophysical experiments.

This emphasis, however, is only part of the story. A man is not always completely related to his environment. Stimulation is not always obtained under the optimal conditions of the psychophysical experiment. On the contrary, the situation is, often enough, one of poor light, faint sound, great distance, or infrequent repetition. Even when the physical conditions are adequate, the eyes may be fixated in the wrong direction. The observation of everyday life is often hurried and fleeting. Even when a man is related to his environment, he is never related to the whole of it. As Titchener long ago pointed out, only a small part of the phenomenal world is ever clear at any one moment of time; perception is selective. There is a limit to the contents of any single percept; apprehension has a span. There is another limit to the duration of any unitary sequence; immediate memory also has a span. The awareness of a man who is going about his business is reduced to those few objects of which his behavior is a function, and to that property of each object which makes it significant for the business at hand. The object tends to function only as a cue for behavior. All these facts can be cited by the psychologists who prefer to emphasize the proposition that perception does not depend on stimulation, but on motivation, attention, organization, or learning.

Both propositions must have validity, for there is experimental evidence as well as common observation to support them. The truth would seem to be that the word perception does not have the same meaning in the two statements. The first kind is not the same as the second kind.

Is it not reasonable to suppose that a man can perceive in two different senses of the term? The first might be called *literal* observation and the second *schematic* observation. The first yields a phenomenal world composed of all the properties and qualities which the man is capable of discriminating. Its nature and limits are established by the psychophysical experiment. The second yields a selected and simplified world, the specific problem-situation relevant to present needs. It can be described by the man for a given hour of a given day, but it may alter radically during the next hour. Its properties depend on the man's motives and on the correlated values of things. One can attempt to diagram this immediate and temporary kind of perception, as Lewin did,[16] but to experiment with it is difficult.

An experimental approximation to this kind of perception is obtained with the tachistoscope. Another approximation is for the experimenter to present many stimulus-objects in succession to the observer. Still another is to reduce the intensity of stimulation nearly to the threshold. It is fair to conclude from a long history of such experiments that in all of them the reproductions, reports, or judgments of the observer tend to be schematic. Visual forms in perception, for example, change from the originals. They are said to be simplified, normalized, accentuated, or conventionalized. In some of these investigations the motivation of the observer has been the experimental variable in producing the changes, or his social attitude, or his past experience, or his temporary set. The latter results are always interesting and often striking, and they unquestionably show that the form, size, color, or other quality of perception is affected by the attitude.[4, 8] It is important to note, however, that the perceptual quality affected is a schematic impression, not a literal one. The conclusion, so frequently drawn from these experiments that perception—all perception—is distorted by motivation is clearly mistaken. The errors that were made would presumably not have been made in a psychophysical experiment. An adult observer in any experiment is usually wary, and he will discriminate with care whenever the stimulating conditions permit. It is on this account that experimenters have to make the conditions unfavorable if they wish to investigate schematic impressions.

The results of experiments with impoverished stimulation certainly do not prove that all perception is distorted and misleading. They do not even prove that *schematic* impressions are necessarily distorted and misleading, if one considers the fact that these experiments are not really good approximations to everyday observation. The distortions involved in the simplified and schematic impressions of everyday experience may not be misleading at all because they are irrelevant to the success of the business at hand, and consequently have no effect on behavior. There is reason to suspect that ordinary observation can usually become literal and exact when the observer needs to discriminate. We are not fated to be victims of our stereotypes nor are we compelled to be deluded by our social norms.

TWO KINDS OF PERCEPTUAL THEORY

The phenomenal world composed of all the dimensions or qualities of experience—sensory, spatial, temporal, dynamic, physiognomic, and social—has been contrasted with the temporary phenomenal world of the problem-situation, composed usually of schematic objects, places, people, signals, and symbols. The literal world is the background for the schematic world. It furnishes a kind of supporting medium of discriminative capacity and it also furnishes a basis for posture and locomotion. The schematic world is a shifting set of prominent meanings from which many of the literal qualities have dropped away.

If there are two kinds of perception, two kinds of perceptual theory are needed. The two theories ought not to be contradictory, but supplementary. Both are required, and they can be developed side by side. The first would be a psychophysical theory of the correspondence between variables of stimulation and qualities or dimensions of experience. Its chief novelty would be the explicit postulate that for every quality of experience there is a discoverable variable of stimulation. This means not only that the sensory qualities, so called, have stimuli, but that all the qualities of surfaces, edges, slopes, and shapes have stimuli, and that all the qualities of motion, action, and causality have stimuli, and that all the qualities of persons, groups, institutions, words, and symbols have stimuli. Its method would be the psychophysical experiment with optimal conditions for discrimination. Admittedly, the isolation and control of the stimulus variables for most of the latter qualities would be fantastically difficult. In this sense, the theory might never be fully verified. It is, however, simply the extension of the accepted theory of sensory experience to spatial and social experience and it provides a straightforward program of experiments.

The second theory would be concerned essentially with the recognition and identification of those things and events in a man's environment which can serve as cues for his action and which he can attend to. In a sense it would be a theory of concept-formation. It would attempt to account for meanings and values, or what the Gestalt psychologists call the structure of cognition. The postulates of such a theory will some day be formulated, although it is doubtful whether any psychologist is capable of doing so now, for the terms needed are probably ones which cut across the present terminologies of learning and of perception. Tentatively, however, a few suggestions may be made as to what a theory of schematic perception might be like.

1. At the outset, it would predict that in two situations identical as regards external stimulation but different as regards motivation, the resulting schematic percepts will be different. An example might be an animal or man who is at one time hungry and at an-

other time thirsty in the same physical environment. Out of the whole range of literal perception which might have been induced by this environment a narrower schematic percept occurs in accordance with the motive operating. A thirsty rat will not perceive the same things as a hungry rat. The schematic percept should be considered just as fully dependent on variables of external stimulation as would be the potential literal perception; the difference is only that one schematic percept is not dependent on the *same set* of variables as is another. The assumption would be that the amount of identifiable stimulation on the receptors at any one moment is greater than the amount the individual can respond to, or perceive, at that moment. The sample of stimulation to which he does react constitutes the cue for his action and determines his schematic percept.

2. Another assumption would be that the number of physical objects in the environment which might furnish cues for action is enormously large, and that they differ from one another by continuous variation. No two physical things are exactly alike, but when the difference between them is small enough an individual reacts in the same way and perceives them as the same thing. Consequently, there will always be a class of objects similar enough to be functionally identical for a given individual. The "discriminating" individual is one for whom this class is small; the "undiscriminating" individual is one for whom this class is large. The latter has never learned to differentiate the stimulus-objects of the class; we say that his reaction to one of them shows "primary stimulus generalization." In

any learning situation, a very important part of the learning process consists of reducing this tendency, that is, identifying the specific cue for a specific response, or acquiring a more differentiated percept of the stimulus-object.[7] The learning of identifying reactions is, then, a prerequisite for all the kinds of learning usually investigated, and it is also a prerequisite for cognition.

3. In this process of acquiring a more differentiated perceptual world, words undoubtedly play a very important part. Verbal naming seems to be the human identifying response *par excellence*. The consequences of this fact for human perception and human learning are extensive. For one thing, naming seems to be a biological invention which vastly extends the range of objects to which the individual can specifically react—it "enlarges" our world as we say. For another, it facilitates communication between individuals; it causes percepts to conform to a social norm, and it tends to standardize percepts in accordance with the verbal names of the language. Probably in this way the identifying reactions of one person are confirmed or reinforced by hearing the same identifying reactions made by another.[19] Perhaps this fact constitutes the main connecting link between the problems of perception and the problems of social psychology. It suggests an explanation, certainly, for the social stereotyping of individual perceptions, and for the influence of culture on perception.

4. The increasing specificity in the naming of objects is only one aspect of the development of schematic perception. As primary stimulus generalization de-

creases, a different kind of generalization increases. The assumption is, in other words, that human reactions manifest not only a kind of generalization based on ignorance but also a kind of generalization based on knowledge. More explicitly, there is evidence to suggest that along with the development of ordinary naming reactions goes the development of qualifying reactions. As individuals learn to identify objects, they learn at the same time to identify abstract properties of these objects and eventually to apply specific adjectives to the specific variables of phenomenal experience. Presumably, this is the fact which makes the psychophysical experiment with human subjects less laborious than the discrimination experiment with animals. The former have *words* for light and dark, thick and thin, smooth and rough, slow and fast.

When an individual can identify the common property among otherwise different objects and specify it with a verbal response, he is well on his way towards conceptual thinking. He becomes capable not only of finer differential reactions, but also of new integrating reactions; he can perceive the similarities as well as the differences among things. From the perceiving of similarities a new kind of naming emerges which enables him to react to classes of objects and events, that is, to form categorical concepts. The new kind of reaction is better called a symbol than a name. In the logician's terminology, the reaction has connotation as well as denotation. The capacity to have symbolic reactions and the experiences that go with them is probably what makes the repertory of human acts so elaborate and the range of human experience so far reaching.

A theory of cognition constructed along the lines indicated above would be applicable to the problems of personality and of social psychology. Implicit in it are predictions about cultural stereotypes and social norms. It should have many corollaries about human communication. It should be able to account for individual differences in the apprehension of social situations. It will account for the seeming fact that we act in accordance with what we "perceive" rather than with what delivers energy to our receptors without implying the subjectivism into which so many students of social perception are led. Above all, the hypothesis that learning to identify things is a basic form of learning and that it constitutes a fundamental type of perceiving seems to be amenable to an experimental test. It suggests a program of experiments on the human naming reaction which might lead towards a genuine science of cognition.

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COGNITION AND MOTIVATION IN PSYCHOLOGICAL THEORY

David Krech

WHEN Professor Dennis invited me to participate in the 1951 University of Pittsburgh series of Current Trends in Psychology, he first suggested as a topic for my discussion: Theories of Motivation. Feeling highly honored by the invitation and wishing very much to participate in this conference, I quickly wrote back to Professor Dennis and told him that I could *not* come to the Conference to discuss "current trends in motivation" because I was of the opinion that the majority of the current theories of motivation are somewhat akin to the 1890 theories of the ether in physics—theories describable as foredoomed attempts to keep alive an already outmoded and unnecessary concept. With this attitude I felt that if I were restricted to a discussion of motivational theory, my style would be definitely cramped and all I could do would be to deliver an unsympathetic funeral oration for the unlamented dead, and this I was reluctant to do. I therefore suggested to Professor Dennis that if he could give me a topic which would allow me a bit more scope, which would permit me to fulfill social expectations and demands—i.e., to be *constructive* and not merely "destructive," I would gladly accept his kind invitation. Because Professor Dennis is an authority on child behavior and on the history of psychology, he displayed a prodigious amount of patience, tolerance, and permissiveness towards my recalcitrance and we quickly agreed upon the present title of my discussion: "Cognition and Motivation in Psychological Theory." However, before plunging into *media res* and developing (or divulging) the theme of my remarks, I feel that a decent respect to the opinions of the motivational psychologists requires that I should declare the causes which impelled me to make the above intemperate remarks about motivational theory. I wish, in other words, to take a *few* minutes out to make explicit what I do and what I do not mean by proclaiming that motivation is (or should be) dead—a proclamation which no doubt many of you feel is highly exaggerated.

Motives, urges, needs, and drives have a long and fairly respectable history among the essential concepts of a well-equipped psychologist. It is such concepts as these, sometimes introduced surreptitiously and sometimes brazenly and challengingly, which have saved many behavioristic and associationistic theories of learning from complete sterility and failure. It is such concepts as these which account, in large part, for the appeal which Tolman's cognitive theories have for so many of us. It is because of Freud's insistence upon our paying attention to motivational factors that many psychologists—those who are favorably disposed to his theories and those who are critical of his theories—regard him as one of psychology's greats. Now I say all of this not with malice aforethought and not with any design to build up the importance of motivation in order later to demolish it more resoundingly. I earnestly believe that the development of motivational theory has helped psychology tremendously. It has helped make our empirical researches more significant and our theoretical speculations more meaningful. It has done this because it has attracted our attention to a whole range of stimuli conditions, psychological mechanisms, and behavior patterns which otherwise might have been neglected. Motivational theory and concepts have helped us to see behavior as organized patterns rather than as concatenations of discrete muscle-jerks; these theories and concepts have forced many of us to pay attention to the developmental history of the behaving organism and to his continuity as a personality over time rather than to the immediately apparent stimulus-response patterns alone; these theories have played a significant role in leading our theorizing from a naive peripheralism into a relatively mature centralism—into speculation about intervening variables and hypothetical constructs. And, finally, motivational theory and concepts have permitted us to make some order and sense out of the apparently chaotic behavior of the sick and disturbed person. All this is true, *but* we must differentiate, I believe, between the assessment of the past *products* of motivational theory, and the assessment of the validity and future usefulness of motivational *concepts.*

In doubting and criticising the validity and usefulness of motivational concepts (and throughout this discussion I shall continue vigorously to doubt and criticise these concepts) I do not for a moment wish to deny the absolute importance of the observations and data which are usually subsumed under the rubrics of needs, drives, and motives. I am not denying, nor do I wish to, that a rat will run a maze towards food more rapidly when he has been deprived of food for twenty-four hours than when he has been deprived of food for twenty-four minutes; or that our experience of female beauty will be influenced by our schedule of sexual gratification, or that people work and strive for this and that goal-object and frequently find integration through such strivings. These are the facts of life—facts which, by the way, experimental psychologists have been very slow to come by. And, I wish to repeat, it is the motivational theorists who have been responsible for pointing out these facts to us. But such terms as "needs," "demands," and "drives" in the hands of the motivational theorists are not merely *names* for the behavior I have referred to, they are not merely linguistic pointers which direct our attention to such behavior; they are labels for intervening variables or hypothetical constructs. Such terms (for all but the most obstreperous of operationalists) refer to something which presumably lies *behind* the observed behavior. A "need" in other words, does not refer to such-and-such *behavior* (i.e., "needful behavior") but refers to something which explains, or helps us understand, or helps us predict such-and-such behavior. And the same is true for a "motive," for a "drive," and for all the rest of the honorable company. And it is the validity and the usefulness of these hypothetical constructs that I am doubting and criticising. I trust, then, that I will not be accused of believing that there is no such thing as motivated behavior. I believe that *all* behavior is motivated. I believe that, I assure you, most passionately.

But if I believe in motivated behavior, so do I believe in cognitive behavior. But here, too, if I see the future aright, the constructs of the cognitive theorists are also on the way out. It is quite obvious, if you examine many of the theories extant in psychology today, that these theories do not rest content with motivational constructs only, or with cognitive constructs only, but require *both* sets of constructs. In addition to their motivational constructs of needs and drives, they also have such constructs as sign-gestalten, hypotheses, habits, association-bonds, cognitions, percepts, reflex-reserves, ideas, memories, and many others. In almost all of these theories, the *relationship* which is assumed to exist among these two groups of constructs is quite a complicated one. On the one hand it appears that these two families of constructs are independently existing ones. On the other hand, it is equally apparent that these independently conceived constructs function in an *inter*dependent manner. Thus, for example, a need or a drive is frequently seen as something which supplies energy for behavior and has *its* functional laws; a cognition is seen as something which directs the energy so supplied into this or that specific channel and has *its* functional laws. And both of these constructs are frequently seen as dynamic "things" or "activities" or "events." Thus it would be possible, presumably, for an organism to "have" a sex need without having the slightest notion or hunch of what he is experiencing or what to do about it. It would be equally possible for the organism to perceive and know a "sex object" without having any evaluative judgment of it as far as his own welfare is concerned.

Now, for most theorists these are possibilities, but they would be quick to point out that such possibilities rarely come to pass—if ever. Actually, they would maintain the need affects the cognition and the cognition affects the need. In other words, needs and cognitions, and perceptions and wishes, and drives and memories *interact*. The belief in the *interaction* among the hypothesized cognitive and motivational processes is certainly one of the most outstanding of the current trends in both motivational and cognitive theories today. This seems to me the minimum lesson we can draw from all the recent and stimulating experiments coming out of the work of Bruner and his colleagues at Harvard, of George Klein at the Menninger Clinic, of Frenkel-Brunswik and her co-workers at the University of California—and of many other laboratories. (For one collection of such studies I refer you to the symposium recently published by the Duke University Press, "Perception and Personality."[1] The evidence for the all-pervasiveness of cognitive-motivational *interaction* seems to be limited only by the notorious publication lag of psychological journals. Everyone is doing it.

It appears to me that this current trend is good, but not good enough. If the theoretician has already recognized that a separatist position is inadequate, i.e., that in the laboratory, in his observation of behavior in the field and in the clinic, and in his attempts to describe all this theoretically, he must pay a *simultaneous* and integrated attention to two sets of constructs, isn't it possible that there is really no need for two different constructs, but that one will do as well? After all, the nature and number of the theoretician's constructs are of his own invention—he does not have to expend an inordinate amount of energy and ingenuity to outwit himself! Why not make things easy for himself and, if he finds that his first invention of two separate kinds of constructs leads him into the difficulties of "interaction" (and a word on that in a moment), then why does he not start over again and produce one construct where two grew before? Now of course it will be argued that this may make his theory more beautiful, but will it work? And this is a very important and serious question. I certainly do not believe that science is nothing but a system of conclusions drawn from definitions and hypothetical constructs and postulates that must be consistent but otherwise may be created by the free will of the scientist. I quite agree with Courant[2] who says:

> If this description were accurate, mathematics (or, I would add, any science) could not attract any intelligent person. It would be a game with definitions, rules, and syllogisms without motive or goal. The notion that the intellect can create meaningful postulational systems at its whim is a deceptive half-truth. Only under the discipline of responsibility to the organic whole, only guided by intrinsic necessity can the free mind achieve results of scientific value.

I repeat: This is an important truth to remember, and any attempt to substitute one system of concepts for another must not only satisfy our desires for beauty and simplicity, but must also pay proper respect to the nature of our problems and data. But at least on the face of it the possibility of reducing our two sets of cognitive and motivational constructs is here, and it is a possibility which must be assiduously examined.

Continued adherence to the dichotomous approach in cognitive-motivational theory can lead, it seems to me, to a plethora of increasingly complicated, tortured, and patchwork attempted solutions. Hochberg and Gleitman[5] in their paper entitled "Towards a reformulation of the Perception-Motivation Dichotomy" discuss some of the difficulties, for example, which "interaction" can lead to when it is used as an attempted resolution of the cleavage between perceptual and motivational processes. They point out that once you have introduced the term "interaction," you have *added* a new concept—and a very troublesome one. If "interaction" is going to be at all helpful in your work, you must have some notion of what interaction is, of how it operates, and of its mechanisms. Anything less than that, I would suggest, is an appeal to word-magic to get you out of difficulties. Their further discussion of this point, I believe, merits extended quoting:

> In maintaining the traditional separation of these psychological processes, one is faced not only with the difficulties of perception psychology alone, or of motivation psychology alone, but with the problems of one multiplied by the problems of the other. It is difficult to conceptualize the common ground upon which "perception" and "motivation" can meet and interact. So long as they are couched in different dimensions, the dualism which had postulated the pineal gland as the point of interaction between soul and body is, at best, but lightly concealed. . . . The compromise view . . . finds itself in an almost more difficult position than did the completely insular one it had tried to replace; the postulation of an "interaction" effect between essentially separate processes has removed none of the problems caused by their separation and may even be said to have added a few. In a way this situation seems

analagous to the history of the Ehrenfels concept of *"Gestalt-qualität,"* a theoretical hybrid that combined both sensations and form elements and found itself under fire from both sides—later to be supplanted by the more thoroughgoing, radical revision of the Gestalt school proper, which completely removed the dichotomy between sensation and perception by denying the existence of the former.

If, as I have already pointed out, one of the current trends in cognitive-motivational theory is the obsession with "interaction," the other, and in my opinion more promising current trend is towards a radical reformulation of the problem—one which denies the validity and usefulness of separate cognitive and motivational constructs, but which seeks to develop a unitary construct which will cover all so-called motivational as well as so-called cognitive behavior. And it is this current trend which I now wish to discuss. I shall do this by briefly outlining five recent theoretical formulations, those of Hochberg and Gleitman, Miller and Dollard, Wolpe, Hebb, and finally my own attempts.

The first of these, the Hochberg and Gleitman suggestion, I have elsewhere[10] characterized as "perceptual imperialism"—an accusation which Hochberg and Gleitman do not deny but seem, rather, to glory in. Essentially, they would seek to subsume the so-called motivational processes under perceptual ones. Their argument goes somewhat as follows: (For the following description I have taken the liberty of abstracting and paraphrasing pages 185 to 191 of their article. I will, however, dispense with quotation marks even though I will occasionally quote their exact language):

The fundamental law of perception is the law of *pregnanz,* and this law is seen as the application of the thermodynamic principles to perceptual processes, i.e., all perceptual processes seek to maintain equilibrium. Disequilibrium is constantly created by the effects of stimulation producing changes in the isomorphic field, and just as constantly the field reacts in the direction of restoring equilibrium. In most examples of this phenomenon the equilibration involved might conceivably take place without overt muscular concomitants, but there are many instances in which perceptual equilibration cannot take place by nonmotoric changes alone. In these latter cases equilibrium is restored by alterations of the stimulating conditions and this, in turn, is brought about by motor activity on the part of the organism. As illustrations of this they refer to accommodation and convergence phenomena in which eye movements are held responsible for the final achievement of visual equilibrium. Now, they ask, is this very different from the case of a child running away from a loud noise or reaching for a bar of chocolate? Not at all, since here, too, a disequilibrium has been created and perceptual equilibration proceeds in the only way possible: the stimulating conditions are changed by the participation of the organism's motor processes. However, the story is not quite as simple as this. They believe that all, or at least most "goal situations" require one more complication in the theoretical postulation. The disequilibria do not occur only between isomorphic representations of objects in the perceptual field, but also between the *perceiver* and the objects in the field. Therefore, they now assume that the organism, *as a cognized object,* may be likewise represented in the isomorphic field, and they can now consider disequilibria occurring between the cognized ego and other bounded regions (goal objects) in the same manner as they treat disequilibria among "objects" alone. This position is, of course, reminiscent of Köhler's suggestion of the dynamic relationship which might be assumed to exist between the "self" and other objects in the field—a relationship which Köhler has characterized with the term "bipolar organization." (See especially his treatment of this problem in Chapter IX of his *Gestalt Psychology*.[9] Thus, motivation is neither superimposed from above nor injected from below, but is an attribute of the total field situation. To summarize their position: This theory would consider all behavioral phenomena (the so-called motivational and the so-called perceptual) as manifestations of equilibratory processes occurring in the psychophysical field, and from this point of view the formally simplest "motivational" phenomenon might be—not the rat running towards the food, not the infant establishing object-relations with its mother—but the more prosaic case of an incomplete circle tending to close.

The above position has much to recommend it, especially its attempt to bring the motoric into relation with the perceptual, but I also have much fault to find with it. In the first place, I don't believe that the Gestaltist's perceptual laws—*pregnanz* or whatnot—are adequate to encompass all behavior. Those laws were formulated with almost studied disregard for the observations of the motivationalists, the clinician, etc. An attempt to force the "hot" facts of life into the artificially-cooled Gestalt laws of perception will not work. Second, as I have indicated elsewhere,[12] I am suspicious of the validity of the concept of "equilibrium" and the second law of thermodynamics when applied to biological systems. Third, I am not at all certain that any useful meaning can be given to the term "psychophysical field"—the matrix in which equilibration takes place. I have recently discussed this at some length[11] and shall not repeat that discussion here except to point out that in my opinion a "psychophysical field" can only be a never-never land for the theoretician in psychology who would work with hypothetical constructs. Finally, I cannot see how their theoretical structure can encompass the observations which suggest that man's motivational behavior seems to be characteristic of the *individual* just as much as of the situation. That is, their formulation leaves me with the uneasy feeling that one could not deal with the observation of the *continuity* and the uniqueness of personality. But whatever the validity of the above criticisms —and of others which could be leveled—the Hochberg-Gleitman proposal has the merit, at least, of attempting to reformulate a major problem of psychology in such a manner as not to tear asunder what experience and observation seem to tell us are one.

The proponents of the second instance of the current trend towards a resolution of the motivation-cognition dichotomy—Miller and Dollard—would probably be most shocked to find themselves discussed sympathetically here, if for no other reason than that they certainly do not consider themselves "cognition" theorists. Nevertheless, they do belong in the present discussion.

First in their *Social Learning and Imitation*[13] (published in 1941) and more recently in their *Personality and Psychotherapy*,[3] these authors propose a theory which can do away with any significant differences between the so-called motivational and the so-called cognitive processes. Because Miller and Dollard seem violently allergic to hypothetical constructs, they attempt, assiduously, to define all of their concepts in stimulus-response terms. But that, I believe, should not give us too much difficulty in seeing what they mean and in translating, if necessary, their terms into the ones I am favoring throughout the present discussion. For them "drives" are "strong stimuli which impel action." That and nothing more. Thus, they illustrate their definition in the following ways:

> Any stimulus can become a drive if it is made strong enough. The stronger the stimulus, the more drive function it possesses. The faint murmur of distant music has but little primary drive function; the infernal blare of the neighbor's radio has considerably more.[3]

And what is the "drive function"? Here they give the traditional answer that the "drive impels a person to respond." Recognizing, of course, that the responses of people are directed or organized or meaningful, they then go on to state that, "Cues determine when he will respond, where he will respond, and which response he will make." But where do these cues come from? Obviously, the cues also come from stimuli. Thus, one and the same stimulus may be a "drive" and a "cue." To quote from them again:

> Stimuli may vary in two respects: in strength and in kind. Thus sounds may differ in loudness and in pitch... The more the two sounds differ in pitch, . . . they are more *distinctive* as cues. As sounds become louder, they increase in drive value . . . a strong stimulus of external origin, like an extremely loud sound or bright light, can have drive value, and hence motivate a person to do something; at the same time it can be distinctive, and hence serve as a cue to elicit a specific response. Apparently the same thing is true of "stimuli" produced by internal states such as hunger; in addition to their impelling or drive function, they may have a selective or cue function.

Now, if we translate "cue" into "cognition" or "knowledge," it seems to me that Miller and Dollard are saying that both the cognitive process and the motivational process can be determined by one and the same stimulus. Since they identify drive with stimulus, and cue with stimulus, then both drives and cognitions may be conceived of as inseparable. However, Miller and Dollard do not quite come to this conclusion. They seem to want to hold on to a distinction, and they do it in this way: It is true that both cognitions and drives are one and the same stimulus, but stimuli can be *analyzed* into parts. That is, any one stimulus may vary in two respects—in strength and in kind. Now, presumably, one can vary the strength of a stimulus and hold kind constant, or vary the kind and hold strength constant. Thus, one might vary the "drive function" of a stimulus and hold its cue function constant and vice versa. This would enable them to make a distinction between drive and cue—or, in our terms, between motivational process and cognitive process. But such an assumption seems to me a questionable one and illustrates nicely the complicated distinctions forced upon those who would seek to retain even a measure of dualism. It is difficult for me to believe that the cue function of a stimulus is unaffected by the intensity of the stimulus—or again, in my own terms, it is difficult for me to believe that the *meaning* of a sound is unaffected by the intensity, but merely by the pitch. If the *unitary* nature of our response to a stimulus is granted, then we can conclude that "drive" and "cue" are not independent functions, but that we have but one function, i.e., a sort of "drive-cue," and as the stimulus changes *either* in intensity or quality, both the so-called drive value and the so-called cue-value change simultaneously. If we now take the next step and play with hypothetical constructs—a game which, as I have already indicated Miller and Dollard refuse to play—then we can say that any effective stimulus gives rise to, or creates, or puts into action an event which is, at one and the same time, both a "drive" and a "cognition," and never do we have a process or event or mechanism which is either one *or* the other. The problem of interaction does not exist.

Now whether or not Miller and Dollard will accept this interpretation, or misinterpretation, of their position I do not know, but it seems to me that this is one possible interpretation of what they are doing. If I may play the role of a prophet, for a moment, I would assert that the historian of psychological theory will see their analysis as consistent with the current trend towards a resolution of the motivation-cognition dichotomy.

The next attempted analysis I wish to discuss—that of Wolpe's—is very similar to Miller and Dollard's. Wolpe,[16] who has recently published several papers on the problem of neurological mechanisms in behavior, starts his analysis of the problem of "needs" by suggesting that, before we can talk about "needs" meaningfully, we must first determine what is *common* to all these things we have called "needs." In order to do that, he suggests that we have to know what goes on *within* the organism in the case of each need. While, he admits, not everything is known about the physiological states underlying the various needs, nevertheless we know enough to say that there is no physiological correlate common to all of them. However, an examination of the *consequents* of the various needs shows that they do have something in common: " . . . they are one and all antecedents of neuro-effector responses." But then, Wolpe argues:

> If being antecedents of neuro-effector responses is the only thing need-conditions have in common, by what are they to be distinguished from "ordinary" sensory stimuli which are also antecedents of neuro-effector responses? The answer seems to be that no absolute difference exists; and if the stimulating effects of "primary needs" stand out in any way from those of other stimuli, it is only in that they tend more often to be *strong* as measured by strength and extent of effector responses. Every sensory stimulus, therefore, has the essential characteristic of a need.

Since Wolpe, unlike Miller and Dollard, does not object to hypothetical constructs, he then suggests that between the stimulus conditions and the effector responses " . . . *there intervenes excitation of neurones* *in the central nervous system;* and to this excitation the term *drive* may be applied."

If we do the same analytic job on Wolpe which I have suggested for Miller and Dollard, it is easy to see again that we end up with *one* hypothetical construct, not two, since the "excitation" which Wolpe has labeled "drive" is also the excitation which many of us would label "cognition," i.e., an excitation or hypothetical construct brought into being, or initiated by, or co-ordinated to a stimulus or set of stimuli.

Since we are now wallowing in the Conceptual Nervous System (as Skinner[15] refers to the Central Nervous System) and hypothetical constructs located therein, it is obviously demanded that we turn to Hebb and his recent publication, *The Organization of Behavior.*[4] A large part of his book is devoted to the problem of "motivation," and keynoting this treatment is Hebb's conclusion that " . . . 'motivation' has been treated not as a distinct process, separate from learning, insight, or attitude, but as a useful first approximation whose main reference is to the temporal organization of cerebral events." If we examine Hebb's definition of "motivation" it becomes obvious that for Hebb the dichotomy between so-called cognitive processes and so-called motivational processes simply does not exist. Again, perhaps the best way to present Hebb's view is to paraphrase, without quotes, his discussion—especially Chapter 8.

Hebb starts his discussion of *motivation* with a recapitulation of his neurological theory of learning, because he believes that an adequate learning theory permits one to treat with the data of motivation without inserting a special postulate in the theoretical structure. Learning, according to Hebb, consists of two stages. The first stage establishes an organization of individual transmission units into a neural assembly, the second stage establishes connections among assemblies. The most general statement about learning, in terms of Hebb's theory, then, would run something like this: Learning consists of a lasting change of facilitations between the activities of specific neural structures. Since, for Hebb, cortical phase sequences (activities among assemblies) become "associated" (i.e., learning takes place), when these cortical phase sequences occur together, it then follows that the experimenter, who wants an organism to learn something particular, must achieve some control of the phase sequence. Now, then, in a single environment the phase sequence may be extremely variable. The experimenter, therefore, uses all the ingenuity at his command to limit the variety of phase sequences which may occur. He does this by controlling the stimulus field. And when he does this, he is, says Hebb, setting up a motivation! That is all Hebb means by the term. Let us quote directly from Hebb:

> The term motivation then refers (1) to the existence of an organized phase sequence, (2) to its direction of content, and (3) to its persistence in a given direction, or stability of content.

Now, since, for Hebb, an organized phase sequence is the hypothetical construct which he identifies with "conceptual activity" (see especially page 181), the above definition can be reworded, without the slightest injury to Hebb's original phrasing, as follows: "The term motivation then refers to the existence of a conceptual activity, to its direction or content, and to its persistence in a given direction, or stability of content." Little wonder, then, that Hebb concludes this part of his discussion with the observation that:

> This definition means that "motivation" is not a distinctive process, but a reference in another context to the same processes to which "insight" refers; it also means that the waking, normal adult animal always has some motivation (because of items 1 and 2 in the definition) though its persistence in any one direction (3) may not be great.

Again, we see that as far as the basic hypothetical constructs are concerned, everything is motivation and everything is cognition and that there is no motivation and there is no cognition.

In my own recent theoretical speculations[10, 11, 12] I have attempted to achieve the same kind of reduction. In many ways my attempt partakes of the characteristics of all the others I have mentioned above. Briefly, it is this: In the first place I suggest that model-building via hypothetical constructs is a valuable procedure for theoretical advancement in psychology. I then assert that if we are to use hypothetical constructs, these hypothetical constructs cannot be behavioral in nature, cannot be located somewhere in a "psychological field," but must be molar-neurological or molar-physiological in nature. Our search then becomes one for the minimum necessary molar-neurological constructs which would permit us to understand and predict all the behavior in which the psychologist is interested. On the basis of both phenomenological and gross behavior observations, study and experiment, it appears to me that the minimum number of such constructs can be one. I am suggesting, in other words, that since in our actual work we have not been able successfully to isolate so-called cognitive behavior from so-called motivational behavior, that therefore there is the very strong possibility that it will be unnecessary for us to hypothesize two types of neurological constructs. This conclusion, it seems to me, is a requirement dictated to us "under the responsibility to the organic whole and by intrinsic necessity." I have dubbed this molar-unitary hypothetical construct with the dubious name of Dynamic System. And I have defined or described these Dynamic Systems as follows:

> The term "Dynamic System" refers to an open system of central neural processes. As such, Dynamic Systems are conceived of as the fundamental units of the brain field. Dynamic Systems are the molar organizations of specified neural events persisting from previous brain activity and of specified neural events deriving from stimulation originating outside the organism or inside the organism. The total brain field, then, can be analyzed into the component Dynamic Systems but no further.[11]

Now I assume, of course, that *every* experience or behavior of man is correlated with, or is determined by, or is congruent with, or is a function of a Dynamic System or a Family of Dynamic Systems. (You may choose whichever phrase you wish depending upon your own brand of philosophy of science. I prefer the phrase "determined by.") This will be true of the experience of seeing a closed circle, reading a piece of propaganda, feeling hungry, or suffering excruciating pain. Each one of these Dynamic Systems will be the product of a multitude of neural influences—neural activities which originate outside the organism (i.e., from distal or exteroceptive sensory stimuli) and inside the organism (proprioceptive, interoceptive, and intercortical "stimuli"). The Dynamic System is *not* the stimulus, obviously, but the organized and relatively enduring *resultant* of all relevant stimuli. This means, of course, that we cannot speak of "types" of Dynamic Systems if by "types" we mean Dynamic Systems deriving from "cue" stimuli as opposed to Dynamic Systems deriving from "drive" stimuli, or "emotional" stimuli, or "gut" stimuli, etc. This, in turn, means that every Dynamic System will vary along the *same* dimensions and have the same attributes no matter what the nature of the resulting experience or behavior. By attributing a common set of dimensions to all Dynamic Systems, I have created the preconditions necessary for interaction among Dynamic Systems. And, of course, communication among Dynamic Systems is essential for my system. But I am not now faced with the same problems of interaction which faces the dualistic interactionist, since my Dynamic Systems *do* have a common ground upon which one Dynamic System can meet and interact with another Dynamic System. Of course, one Dynamic System will differ from another, but not in the number and kind of attributes or dimensions, but rather in the various weightings given to those attributes. All of this means two things as far as our experience or behavior is concerned: (1) So-called motivational and cognitive attributes will be involved in every one of our experiences and behavior, and (2) the so-called motivational attributes of any experience will be intimately related to all the other attributes and will change as they change. If we wish to speak of a "hunger motive," for example, we must simultaneously speak of the so-called perceptual, cognitive, memorial, and other attributes of the experience. My "hunger" is not the same as yours, because my Dynamic Systems *re* food and eating are not the same as yours. And neither you nor I can have any "pure" sex motives—they are both sullied by cognitive factors.

I have suggested, above, that Dynamic Systems are relatively enduring structures. That is, every one of you has a brain full of Dynamic Systems—Dynamic Systems relating to triangles and women, to science and magic, to paintings and fences, to chairs and professorships, to cabbages and kings—and you bring these Dynamic Systems with you whenever you enter any situation and your behavior reflects these Dynamic Systems as changed by the immediate situation. Thus, whether you are discussing behaving with respect to women or science or paintings or chairs or cabbages, your behavior will show both variability and consistency in its motivational and emotional *and* intellectual aspects. These words—motivational, emotional, and intellectual—refer to different common-sense ways of describing this or that aspect of your unitary experience, and *may* refer to genuine and analyzably different aspects of phenomenological experience. Now, any one of these aspects may occupy the center of consciousness at any one time, but *behind* all of these experiences we need postulate only one kind of hypothetical construct which generates all of these partial experiences. I am asserting, you see, two things: First, I am asserting that the hypothetical constructs of a theory need not be as pluralistic as the variety of our phenomenological experiences. The multitude of attributes which we can isolate phenomenologically may be represented by variations along one or more dimensions of a *single* hypothetical construct—the Dynamic System. Second, I am asserting (and here I am paraphrasing Köhler[8]) that although all phenomenological events have correlates *within* the unitary Dynamic System, it need not follow that every phase of Dynamic System activity is represented by corresponding phenomenal facts of equal intensity. In other words, the context of Dynamic System events may be wider and functionally more continuous than is apparent in directly accessible experience. But this is neither the place nor the time (nor, I must confess, do I have the wit) to spell out the neurological attributes of these wonderful Dynamic Systems in detail. I have recently published a first attempt in that direction[12] in the November, 1950, issue of the *Psychological Review,* and those of you who are interested are referred to that publication.

The implications for *theory* of my Dynamic Systems, or of Hebb's cell-assemblies, or of Wolpe's Central Neural Activity, or of Miller and Dollard's drive and cue stimuli, or of Hochberg and Gleitman's equilibrating psychophysical field are pretty much the same as far as the problem of the motivational-cognitive dichotomy is concerned. I believe that I have already discussed that in sufficient detail. But it may be of value to summarize briefly the implications of all of these theoretical approaches for *empirical* work. The major implications in this connection, it seems to me, are two in number:

(1) The first guide which this theoretical approach suggests is that, in summarizing the results of all of our experimental work, we may make greater advances if, instead of seeking to derive from our data such segmental laws as "laws of perceptual behavior," or "laws of learning behavior," or "laws of motivated behavior," we attempted to derive "laws of behavior." Experimentally this would mean that we could not talk about "varying the stimulus conditions and holding motivation constant," or "varying motivation and holding knowledge constant." Varying any one set of significant stimuli will vary *all* the attributes of behavior and experience. Much the same point has been made by Klein[7] in his provocative paper "A Clinical Perspective for Personality Research" in which he calls for some new thinking on the problem of experimental design. While Klein sees the need for such new thinking as deriving from the "clinical attitude," I believe that this need is also a consequence of the current trend in motivational-cognitive theorizing among the experimental psychologists I have discussed. Thus I would agree completely with Klein's statement that an experimental situation which attempts to hold constant neighboring variables often prevents the appearance of significant relationships, and I sympathize with the clinician who, says Klein, " . . . can justifiably reject much of the experimentally derived 'laws' of learning, perception, or other functioning on the basis of the

neglect of this principle," but would only add that the theoretician, too, can justifiably reject many of these partial laws.

Now, the above position does not mean that we can no longer analyze behavior, or vary experimental conditions, or observe correlations between experimental conditions and behavior—these operations are basic to experimentation—but what it does mean is that we must re-examine our ways of doing these things. A promising first approach to such a re-examination, by the way, can be found in Holt's paper "Some Statistical Problems in Clinical Research."[6] And that leads me to the second implication for empirical work.

(2) This second implication is that we must develop new descriptive units of behavior, more catholic approaches to the description and control of our experimental conditions, and, perhaps, even new procedures of statistical analysis of our data. Instead of describing behavior in terms of the common sense and dichotomous categories of effort, speed, successes, errors, emotional outbursts, etc., we may discover that such terms as "rigidity," "complexity," "intensity," etc. are preferable. Admittedly, this is only a guess, and in so far as it is only a guess, I cannot claim to have a "theory" here, but only an aspiration towards a theory. For I agree with Willard Gibbs, who said in his letter to the American Academy of Arts and Sciences on the occasion of his acceptance of the Rumford Metal, that " . . . it is the office of theoretical investigation to give the form in which the results of experiment may be expressed"[14] and that I cannot now do adequately. All I am doing is *pointing* to the problem and aspiring, but

the problem is there. Similarly, instead of giving a *partial* description of the experimental situation, we must seek for as *complete a description as feasible*. No matter what experiment we are doing, we must seek to describe the so-called "internal" *and* external stimuli, or the so-called physical *and* social. And this holds, I repeat, whether we are doing a rat experiment, a perception experiment in the dark room, or a Rorschach study. And, finally, as far as our new statistical procedures are concerned, I can do no better than to quote Holt's almost pathetic plea to the statistician—a quotation I take from his paper referred to previously:

> Perhaps we are asking for the impossible when we say that we would like means to handle the simultaneous variation of many variables, sometimes curvilinear, sometimes discontinuous, and relationships between patterns, all with relatively small numbers of cases and preferably without the necessity of too much computation, please.

One final word by way of apology, of summary, and conclusion. In Professor Dennis' directive to us, he pointed out that "we envisage our conferences on current trends as differing from other psychological meetings in that our meetings attempt to evaluate the present situation in some area, and to consider its probable future development, rather than to review the history and research data of the area." I have interpreted this to mean that by virtue of the authority vested in Professor Dennis, I was hereby permitted to don the mantle of the prophet, survey the present, and to take for special examination only those trends in cognitive-motivational theory which, *in my opinion,* were blessed with the promise of a long and fruitful future. This I have attempted to do, and therefore, quite obviously, I have run the risk of being completely wrong—a risk which every would-be prophet must run. My crystal ball may have been very very clouded indeed. You will have noted, no doubt, that I have altogether omitted any discussion of the most recently proposed lists of "basic drives" and "second-order drives," or the most currently favored view with respect to the relationship between the so-called biological drives and the so-called social drives, or the current trends with respect to the problem of the rechanneling of drive-energy, or the current controversies over reinforcement versus contiguity theories, or association-learning versus cognitive map theories. I have even neglected to give empirical chapter and verse in support of the theoretical approaches which my prophetic vision has singled out for a long and fruitful life. I have neglected to do this latter chore because I do not believe that we have, at this time, conclusive experimental evidence for the validity of this approach. At this stage of the game I am willing to rest my case with the statement of *faith* that the search for a single hypothetical construct which will do away with the cognitive motivational dichotomy represents the most promising current trend in theorizing in psychology. Whether any one of the five specific attempts in that direction which I have discussed today is the correct one is not of immediate significance. What is significant is that the theoreticians, faced with the problem of reconciling two sets of bothersome constructs—constructs which they have inherited from their elders and perhaps betters—are not afraid to re-examine their inheritance and the assumed starting point of all theory. They are showing full cognizance of the fact that, to some degree at least, hypothetical constructs are made to serve science and not the other way around, and are therefore in the unique position to take the advice of the Vermont farmer who, when asked by a motorist how to get to St. Johnsbury, pondered awhile over the complexity of tortuous detours and turnings involved and ended up by saying, "Well, if I were you, I wouldn't start from here." And this fresh reorganization I like; and what the prophet likes, he prophesies.

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THEORIES OF PSYCHOTHERAPY

Frames of Reference in which Psychotherapeutic Events Have Been Conceptualized

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WITHIN the past decade there have been a number of books published presenting the data and theories of psychiatry and psychotherapy, systematized from several points of view. To attempt to classify these books or their authors into separate schools—such as schools of psychoanalysis,[1, 2, 5, 9] psychiatry,[3] dynamic psychiatry,[11] non-directive therapy,[15] interpersonal relationship theory,[18, 19] psychobiology,[12] etc.—is probably more of historical than of operational interest.

A few months ago Dr. Frieda Fromm-Reichmann's book, *Principles of Intensive Psychotherapy,*[4] was published. In this book Dr. Fromm-Reichmann presents, with a wealth of clinical evidence, her own elaboration of the concepts and theories developed by Sigmund Freud from the point of view of energy and biological instincts, and of the concepts developed by Harry Stack Sullivan from the point of view of relationship and communication. In addition, the principles evolved are applied to the group setting in which psychotherapy is conducted, reflecting a current trend which may well be the next area to be intensively studied, namely, the role of the group.

In view of the fact that Dr. Fromm-Reichmann's book summarizes the major current trends in the theories of psychotherapy, together with the practice on which the theories are based, I wish in this paper—rather than reviewing the material—to call attention to and to make some comments on certain similarities in all psychotherapeutic systems, particularly with reference to their group significance. By way of introduction I wish first to consider certain aspects of the phenomena referred to by the word "theory."

Theories, like language, are central nervous system activities which have two functions, one of which is highly economical for the single organism, the other for the group. In the single organism this central nervous activity, which apparently may involve only a part of the available functional tissue, is capable of organizing the total function so as to predictively orient the organism in both space and time and also to determine to a greater or lesser extent the attitude in the situation. This obviates the time-consuming process of scanning each new situation and its probable course, but only requires sufficient scanning to determine which theory is to be adopted. The second function of theories—and of language—is the hortatory function, of similarly orienting another person. The adequacy of a theory in this respect will depend on several factors, two groups of which may be noted here: (1) the precision with which the theory differentiates the relevant factors in the situation, which include the operations of the observer on which the theory is based, and (2) the capacity of the group to whom the theory is addressed to perform the operations represented by the theory. In other words, a theory is intensely personal, being a personal performance resulting from repeated experiences in participant observation, and at the same time it is intensely interpersonal, being determined by the relations of the original observer with his colleagues and by the interpersonal transactions of the group as a whole.

The further evolution of a theory likewise depends on the nature of the group using the theory as a means of communication and is also necessarily influenced by the status of this group in the larger group of which it is a part. It is unfortunate that these interpersonal group factors have not been better documented during the development of the theories of psychotherapy in the first half of this century. One gets only inklings of them in occasional intimate conversations with people who have known the chief investigators in the field.

In so far as theories are central nervous system functions for predictive orientation they are dependent on the earlier functional experience of the organism. The earlier experience thus largely determines not only the differentiated parts of the total function, e.g., the particular words in a language, but also the general attitude, or set, or conceptual frame of reference, e.g., the structure of a language. These previous experiences in the case of humans are mostly interpersonal. It was not accidental, then, that Freud's theory of psychotherapy, and the theory of personality derived from it, was part of the language of science in which he was trained, a language of fixed time and space, energy mechanics, and biological absolutes. Nor was it accidental that Sullivan's theories were cast in the form of related time-space, field forces, and communication—the early twentieth-century language of the physical and social sciences. In addition, a person trained to predictively orient himself and communicate in one conceptual frame of reference cannot adequately use another without training in the general attitude as well as in the precise words. It may also be noted that one may without too much difficulty translate a theory from, say, German into English, but one cannot similarly translate between two theories based on different conceptual frames of reference. It is necessary first to determine the common denominators in the operations symbolized.

Psychotherapy, as a differentiated class of operations subject to investigation and free communication, is a very recent development and so may be regarded as being young. It may therefore be useful to consider the early significance of the words from which the term is derived, that is, the operations communicated by *psyche* and *theraps* when they were young. I propose here to translate freely, not on the basis of accurate philological knowledge, but rather on the basis of impressions derived from my experiences with colleagues and patients. *Psyche* derives from a word originally connoting "to breathe" and hence came to refer to life in contradistinction to death, in which breathing stopped. When the functional organization which prepares for its own continuity in the future breaks down, death ensues. Thus, life is characterized by interaction, not in terms of the immediate present, but in anticipation of the future. *Psyche* came to be used to differentiate this temporal quality in interpersonal and intergroup relations, and we have *psyche* referring to "the soul." I propose to define "the soul" as "the sanctity of the person as an integral part of the group," in an attempt to formulate the operational significance of the use of the noun. I use the term "sanctity" to imply both the life-or-death importance of the soul and also the sense of awe at the unknown, particularly the sense of awe some people experience on discovering that the welfare of one or more other persons has become as important to them as their own. The concept of *psyche* as referring to "mental" is still later. Here the operational formulation may well be somewhat as follows: "The learned tricks whereby we establish our cultural status, with only indirect reference to our group dependence." This significance is of the post-Renaissance period, a derivative of the megalo-manic attitude that man can do anything if he has power enough, and that power is to be obtained by the manipulation of the environment.

The word *therapy* derives from the Greek *theraps,* meaning "an attendant, or servant" and thus fits the Hippocratic concept of the physician as one who served the patient in terms of his knowledge of the probable course of the illness, at the same time recognizing that he did not know the cause of the malady. It may also be noted that in folklore the good servant is always one who attends to his master's future benefit, regardless of the current demands or orders. Further, the servant was not defined in terms of a specific master, and throughout recorded history the therapist has been the one international and interracial role.

We may then regard psychotherapy in terms of attending or serving a person with regard to his relations with the group, which relations are only satisfactory when current performance reliably predicates stable continuity of communication. If we disregard questions of authority and delimitations of groups, we find that there are certain common denominators, either implicit or explicit, in all the systems men have developed for living-togetherness. I wish to consider certain of these factors here as they are illustrated by current theories of psychotherapy, which are particular cases of living-togetherness.

In all psychotherapeutic systems there is the problem of the limitations of the applicability of the methods and the associated problem of the extent to which the methods may be modified to meet new situations and still be consistent with the theory. It is around the latter question that the more bitter and less productive polemics arise. If we consider psychotherapy in a broad sense, however, it is clear that we do not yet know what the limiting factors are, but it is evident that many situations previously held to preclude therapy have proved not to do so. Thus it has been found that it is not necessary for a patient to pay for his own treatment. Also, it has been repeatedly shown at the Chestnut Lodge Sanitarium that it is not necessary for a patient to come to the physician and ask for treatment. In many instances successful therapeutic relations have been established with patients who were forced or even tricked by their families into the hospital.

The range of intelligence over which psychotherapy may be effective has not been fully investigated and the degree of brain injury which precludes psychotherapy is not known. At intervals over a number of months I have been discussing the course of treatment of a frontally lobotomized schizophrenic patient with a colleague who is conducting the therapy. There is no question but that the lobotomy has introduced a very considerable hindrance in the capacity of the patient to establish relations with other people and consequently to develop more effective interpersonal operations.

The course of psychotherapy with this patient has followed the course we have learned to expect in treatment with schizophrenic patients, though more slowly and with greater difficulties. There is no question but that considerable improvement in performance and insight has occurred. A certain haphazard quality remains, however, more or less constant. It may be that another method, more resembling the supportive education and training used with post-encephalitic children, will be necessary as an adjunct. However, it may be that if we can learn what the modifications in interpersonal transactions following frontal lobotomy are, we will be able to assist the patient more adequately.

The genesis of psychotherapeutic systems is similar in that these systems are associated with particular men who in many ways may be considered as rebels—that is, they have rebelled against certain aspects of their cultural patterns, but retained others. Thus Freud rebelled against certain cultural attitudes towards biological phenomena such as sex, but retained the form of thinking of energy mechanics in his concepts of instincts as forces. Sullivan rebelled against the energy mechanics of classical physics but retained certain concepts of tension and field forces in his theory of interpersonal relations. It is to be noted, however, that Freud introduced first approximations of concepts which have since been elaborated in other terms. Thus, he described the ego in terms of processes, not in terms of a structure, and he prognosed that the concept would undoubtedly come to be used in a fixed structural sense rather than in the sense of continuing, changing functions. The operations symbolized by Freud's concept of ego are referred to as the syntaxic mode of experience in the interpersonal relationship theory of Sullivan and may be expanded in more quantitative terms in communication theory. Sullivan rebelled against the energy mechanical system of Freud's love and death instincts and introduced field theory concepts of security in the social setting and the breakdown due to anxiety. It now appears as though these concepts may be formulated as general properties of all servo-mechanisms, i.e., of mechanisms the functioning of which is guided by information received during the function, which information is in part or entirely determined by the function. Such mechanisms function more precisely the more accurately return coincides with the set of the system ("love" and "security"), but when inaccuracies occur the system becomes less stable and if one part is overloaded the breakdown feeds back into the system in such a way as to result in further breakdown ("death" and "anxiety").

In mechanical and electrical systems the designer builds into the system further controls which stop the whole operation. Since in the community of living organisms the system cannot stop, there have been developed control mechanisms with strong negative feedback as stabilizers. In social groups these are referred to as stubbornness, in psychoanalysis as resistances, and in interpersonal relationship theory as anxiety operations for defenses. Whatever the terminology and its other implications, the common denominator of the functions referred to is to decrease the range over which interaction may vary. From the standpoint of the therapist who is directed towards increasing the range (whether it be in one direction or in all directions) these stabilizing functions are "bad" and have to be "worked through."

We may note that in the sequence, social groups, psychoanalysis, interpersonal theory, and cybernetics, we have more and more abstracted ourselves from the situation of the elaborate obscurity of intimately living with another person. We may also note that various forms of group function are reasonably adequate to meet the needs of the great majority of people. Psychoanalysis has proved useful for many neurotics who are not too deviant from the culture. The theory of interpersonal relations developed in conjunction with the treatment of more deviant people, namely, patients in schizophrenic states, and indeed is regarded by some as applying only to the treatment of such patients. I have found the more general formulation in terms of interaction and communication of value in reducing anxiety concerning psychiatry in a number of physicists, mathematicians, and chemists of my acquaintance. I have also found the conceptual framework of information theory useful in calling new problems to attention, together with ways of investigating them.

In all psychotherapeutic theories the therapist is enjoined to deal with the patient in terms of the patient's personal experiences and not in terms of the general theory. This is variously formulated, but it follows from the observation that the theory is a function of the therapist's relations with a group, whereas psychotherapy is the development of the transactions between the therapist and the patient.

The theories which in my experience have led to difficulties most frequently have been those concerning early development. It is easy to consider life history with a patient in a theoretical frame of reference without using technical language. Further, life history is often a relatively safe medium for communication since it is remembered in terms of current defenses against anxiety, or as rationalizations of current difficulties. In several instances in the course of treatment I have found that conclusions arrived at early in treatment on the basis of widely accepted psychiatric theory later had to be revised, in fact, almost reversed. For example, an episode of a boy disobeying his father repeatedly and precipitating at last a sound thrashing with a belt at first was regarded as an experience of competition and rejection. Later it proved to be an example of the boy's confidence in his father who was one of the more benign figures in his life. The father was a very aloof person and the apparent antagonism of the child was a move towards establishing some reliable intimacy. It was much better to be spanked than to be disregarded as a person, and the experience was not one of rejection or competition, but rather of personal reliability and importance. To understand the significance of life history we have to know what happened, its time sequence, and the setting, not only of the relations of the child with parents and siblings, but also the relations between the parents and between the family and its social subgroups. Life history can be most useful in working out the genesis of current problems, but is much safer so used when the current problems have been reasonably adequately delineated.

All psychotherapeutic systems advocate, and many require, particular training of the therapist but except the originator of the system from such requirement. The training necessary for the therapist is a very complex problem and still far from being solved, although at present it is receiving possibly more attention than any other problem in psychotherapy. Quite apart from learning technical details and recorded facts, the therapist is expected to undergo the experience of therapy himself. The theory that one cannot deal adequately with another person's problems if one's approach is complicated by one's own problems is considerably clarified in Sullivan's formulation of "participant observation." The practical application of this principle is presented at considerable length by Fromm-Reichmann who re-emphasizes a very important aspect of psychoanalytic theory; namely, that the therapist must always be aware of personal problems complicating the therapeutic relationship and must be ready to undertake further therapy to solve the complication. In other words, the therapist's therapy can never be considered as complete. Indeed, it is only in those systems in which the therapist is playing a ritualized role that the concept of completed preparation of the therapist is entertained. In such systems the therapy is found to be applicable to only a narrow group of interpersonal problems and frequently results in both the therapist and patient being dependent for their security on a subgroup in the culture which is relatively very small.

The theory of preparation of the therapist may be ideally stated rather simply, but the complexity of the problem becomes manifest in its practical application. Probably the best criterion at present for judging the adequacy of a therapist's personal training is the opinion of a group of more or less trained observers, preferably of different personality types, on the question of the therapist's ability to recognize and constructively avoid relationships with patients in which the therapist's personal problems will seriously interfere. In other words, the decision is on the lower end of an ideal scale, namely, that the therapist will be unlikely to do harm.

The negative character of the foregoing statement is more apparent than real. This is due to the fact—as Sullivan phrased it—that "the tendency to health is greater than the tendency to illness." We do not need to do things to make the patient well; all we need to do is to understand and respond to the long-term purposes of the patient's operations and not get in his way by misinterpreting the short-term maneuvers that occur in the course. Stanton's and Schwartz'[17] observations on excitement states offer a good illustration. The excitement is not a phenomenon we have to do something about per se, but rather a desperate attempt on the part of the patient to communicate the existence of a real and intolerable situation, namely, an anxiety-laden disagreement between two responsible authorities. If the therapist understands the patient's communication and responds by relieving the threat of the real situation, the patient can proceed. He may then eventually develop more adequate capacities for differentiating the various factors in a similar situation and simultaneously more adequate capacities for reasonable self-assertion.

It may be noted here that with the hospitalized, psychotic patient the therapist often "keeps out of the way" by manipulating the real situation. In doing so he shows consideration and respect for the patient's actual lack of capacity at the time. This is less frequently the case where the patient's interpersonal operations are such as to permit living outside a hospital. However, in hospital practice as well as in office practice, the therapist may get in the patient's way by manipulating the real situation and so confirm the patient's worst fears, namely, that he cannot deal constructively with other people.

The principle of the tendency to health is generally held in psychotherapeutic systems and enters into a number of theories. Thus, it enters into the concept of growth of the person and of mental illness either as the interruption of growth or as regression to an earlier form of interpersonal transactions. This has been one of the most fruitful theories in the development of psychotherapy. As with other theories, certain implications have not yet been adequately investigated, whereas others have been grossly overworked. Thus, a great deal of attention has been directed to the current expression, and the genesis, of destructive and inadequate forms of interpersonal relations. Insufficient attention, however, has been paid to the fact that some patients are mutely and hopelessly convinced that they have no capacities for developing relationships of fondness, tenderness, or other pattern promising stable continuity. These patients show no awareness of memories of experiencing or feeling such capacities, nor do they know how their conviction of innate inadequacy developed.

Many psychotherapists hold the view that what we call "positive" behavior will occur as soon as it is released by removal of blocks, resistances, incorrect anticipations, hostilities, anxieties, and so forth. Janet Rioch[14] pointed out the value to a patient of recalling at one time having clearly demonstrated the capacity for positive feelings and performance. In my own observations on patients in manic states I have noted reduction in the excitement following experiences in which it seemed as though the feelings and the behavior coincided. The patient then did not have to be so busy play acting the feelings he thought he ought to have, but which he was convinced he did not have. I do not think we can necessarily assume that a capacity for any particular type of interaction is present in the absence of evidence that it has at one time functioned. The anatomical substrata are no doubt present and intact, but the development of function in some instances requires experience. In such instances it may be necessary to provide opportunities over a considerable period of time to develop personal relations for which the early environment provided no experience. This problem is subject to investigation both in the therapeutic situation and in animal experimentation. It seems to me that amongst the most important experiments performed in the first half of this century are those of Lashley and Russell,[10] and later of Hebb,[6, 7] on rats and those of Riesen[13] on chimpanzees, in which the animals were reared in darkness and when relatively mature brought into the light. The results confirm and experimentally extend the observations collected by Senden[16] on humans with congenital cataracts operated on later in life. Hebb[8] points out in his monograph that in all three species the figure-ground relationship was responded to on the first experience, but that interaction with the figure in terms of its shape or pattern required increasingly long experience in the more complex species. We may quite reasonably speculate as to whether in the complex patterns of interpersonal relations a person without early learning may respond with a general alerting to an interpersonal situation, but may require long experience to develop the capacity for adequately differentiating and responding to the structure of the situation.

Where previous experience has occurred, the problem of psychotherapy would seem to be quite different. Here it seems quite evident that the resolution of interfering factors, the reassurance of capacity for continuing communication, the reintegration in a group, will release the capacities for stable interaction and permit them to continue to mature. In this regard it is important to note that the fact of continuing communication with the therapist is of equal or greater importance than the content of communication. The relationship with the therapist may be regarded as the development of a two-person group, the reliability of which the patient must necessarily test. In this testing procedure he is not so concerned with the reliability of the therapist, though that is what is often verbalized.

He is more concerned with the reliability of his own predictive performances. With reasonable anticipation of the reliability of interaction in the two-group situation, a patient will frequently be able to utilize real current experiences with other people for extending his capacities in human relations.

The theory that all of the performances of a patient with a therapist are in terms of patterns of his earlier life has been of great value in relieving the anxieties of the therapist and often of the patient, and with the relief of anxiety, permitting a more mature and comprehensive consideration of the problem. This theory, however, has been grossly overworked. I have not known of any instance of a psychotic or neurotic patient responding in a situation to something which was not there—though frequently the observer has to scrutinize not only the immediate situation, but its time-course and his own participation, to determine that aspect to which the patient is responding. Many observers have interpreted hallucinations as evidence that psychotic patients were responding to something with no representation in the real environment. It is to be noted, however, that it is the occurrence of the hallucination and not its form or content which is the response to real factors in the situation. Hallucinations, like delusions, obsessions, anxiety attacks, and other "symptoms" are at the same time evidences of anxiety and defenses against increasing anxiety. The transference phenomenon or parataxic distortion is distortion of the frame of reference in which one or a small group of factors becomes heavily weighted and seems to comprise the whole. The weighting of these factors is apparently determined by the prediction of the patient's own response to them and its expected consequences. What such a patient needs is the solution of the problem which provokes the distortion, and when the therapist shows understanding of this problem the distortion is reduced and frequently vanishes. When the therapist does not understand the problem underlying the distortion, it is necessary to elucidate it in the general setting of accepting the patient and his distortion.

Clues as to the nature of the problem which results in distortion may be obtained from study of the distortion and its historical significance in the patient's life combined with study of the current situation and its setting in a sequence of situations. The study of the current situation includes as one of its most important factors careful scrutiny of the therapist's own form of participation with the patient. It is frequently very useful for the therapist to discuss his own attitudes in a particular situation with the patient. This is done not in the sense of apology or in the sense of burdening the patient with the personal problems of the therapist and their explanations. It is rather to simply clarify the real factors the patient is dealing with in the interaction with the therapist. I have found that when I am not comfortable enough to discuss such material with the patient, it is time for me to ask a colleague to review the therapeutic situation and my role in it with me.

This leads to consideration of another characteristic of all psychotherapeutic systems—the recognition of the importance of the attitude of the therapist to the patient. The general formulation is that the servant shall not make use of his master. That is, of course, only a partial statement, as we are dealing with a relationship and, if nothing else, the master supports the servant with food and shelter. In psychoanalysis, and more so in interpersonal relationship theory, attention has been paid to the phenomena of counter-transference and the utilization of such phenomena in the psychotherapeutic process. Ordinarily counter-transference is referred to in situations in which the therapist feels lustful, feels competitive, is infatuated with, is angry with, needs to gain the approval of, and other such responses, with and to the patient. In my own experience, and from experience in discussing cases with colleagues, such attitudes towards patients occur very rarely, if ever, in response to characteristics of the patient or the situation which represent former problems or transactions in the life of the therapist. Most often the development of such short-term patterns of interaction with the patient represent an attempt to deal with anxiety aroused in the therapist by some unclear problem in the therapeutic situation. Thus, the phenomena commonly referred to as counter-transference are usually the results and not the causes of difficulties. However, in so far as they are inadequate forms of relationship with the patient they may well lead to further difficulties. The study and solution of the course of the therapeutic relationship requires, in this regard as well as in regard to transference phenomena, consideration of long time-sequences and also of current events in the life of the therapist. Not infrequently anxiety in the therapy situation arises from problems in the relations of the therapist with his colleagues. When the therapist's security in his group status is threatened, there are very frequently repercussions in the patterns of relationships with his patients. This is in part related to the fact that one of the commoner forms of counter-transference is that in which the therapist is playing the role of "what the therapist ought to be" and is fitting the patient into the "proper" theory. When the patient rebels at this depersonalization, either by developing desirable or undesirable symptoms, something is bound to happen in the response of the therapist. It behooves therapists, particularly those who use intellectualizing as a means of relating to others, to be ever on the alert in the use of theoretical concepts. Theories as tools for probing into the obscurity of interpersonal relations can save valuable time, but used as solutions to obscure problems they can set up other and even less clear difficulties.

The last characteristic of psychotherapeutic systems I wish to comment on here is that they all attempt a theory of the nature of the psychotherapeutic process. These theories are related both to the operations of the therapist which he may be able to communicate to his colleagues and to the concepts of the nature of personality current in the system. Thus, different systems may formulate the psychotherapeutic process in such statements as: "Replacing bad habits with good ones"; "Making the unconscious conscious"; "Reliving infantile experiences"; "Using free associations to render the unconscious accessible"; "Bringing to awareness unrecognized aspects of interpersonal relations"; and others. Such statements do not refer to the psychotherapeutic process itself, but to theories of the methodology of psychotherapy. We have no theoretical formulations of the nature of the psychotherapeutic process, just as in neurophysiology we have no formulations of the manner in which durable changes in the function of the nervous system are brought about by the functioning of the nervous system. Speculations as to the growth of new anatomical connections between neurones or as to changes in complex protein molecules controlling synaptic thresholds represent possible mechanisms for durable change in function, but experimental bases for such speculations are virtually entirely lacking. Further, to construct a theory of psychotherapy it will be necessary not only to provide a formulation of the nature of durable change in function, but also to state in which of the central nervous mechanisms such changes occur. The problem is probably essentially the same as the problem of learning, conditioning, memory, etc., with attention to difference in content rather than to difference in mechanism.

It is increasingly widely recognized, however, that a great deal goes on in psychotherapy which is either quite unknown at present or is vaguely referred to in terms such as "rapport" and "empathy." The states indicated by rapport and empathy are not dependent on a common background of experience of therapist and patient, on similarity of emotional patterns, or on recognizable feelings of fondness or concern. It has occurred to me that time sequences may play an important role in these phenomena. While working with the two patients who changed most rapidly from psychotic states to being able to live at home, I noted that I most frequently had the experience of coming to a conclusion with regard to a recent episode at the same time as the patient came to a conclusion, more or less similar. With these two patients I also noted less interference with our work by my own anxieties and defenses than with my other patients. In addition, it not infrequently occurred that an experience the patient had with me was followed by improved performance with the hospital group. In contrast, the patterns of interaction were quite different with the patients who changed most slowly in the course of their work with me. We rarely came to conclusions, either similar or divergent, about the same episode at the same time. I was frequently confused as to the course of the relationship and also frequently noted inadequate countertransference patterns. Improvement in communication with me more often followed, rather than preceded, improved performance with the patient's subgroup. It may be that the time course of mental processes is sufficiently similar in certain people to facilitate communication and sufficiently dissimilar in others to interfere with it.

Whatever the basis of rapport and empathy may be, it would seem that the important factor in therapy is the reliability of the communication which occurs in these states. This is not dependent on the content, whether it be antagonistic or friendly, about sex, money, or hostility, nor on the form, whether psychotic or neurotic. The reliability of communication is represented by the accuracy and consistency with which the relevant factors in the situation are determined and responded to, both in the verbal and nonverbal forms of communication, and in the short-term and long-term patterns of interaction.

Another factor in the process of psychotherapy is the group, and the development of a sense of group belongingness. Group belongingness may be defined as the reliable expectation of continuing communication in a stable manner, and the lack of a sense of group belongingness is the threat of doing something which will result in excommunication. For many hospitalized psychotic patients, excommunication is tantamount to extinction as a person. For most ambulatory patients there is usually some expectation of being able to find some other group. A number of studies have been made in hospitals on the use of the group structure in psychotherapy and on the role of the group and the subgroups. Much more needs to be done, however, particularly in view of the current threats to the stability of all social groups. Little has been done on the problem of the role of the patient's subgroups in psychotherapy with ambulatory patients. All my ambulatory patients have reported current experiences which were strongly therapeutic, particularly when reviewed in detail in their total setting. In a number of instances the patient has become aware of both personal problems and personal capacities only after an experience with a friend or a relative, and this has occurred in areas of interpersonal transactions which were not being dealt with at the time in the psychotherapeutic interviews. Such phenomena offer an explanation for the fact that a considerable diversity of psychotherapeutic systems are to a greater or lesser extent effective. It may well be that, provided the therapist affords a reasonably stable two-group situation, the patient can utilize other groups for developing increasingly stable forms of relationships. It may be that more therapy than has yet been imagined can be accomplished outside any particular psychotherapeutic situation when we come to understand the role of the group.

The comparison of the results of various psychotherapeutic methods and of modifications of methods is at present extremely laborious, if possible at all. The practical test of relief of symptoms and complaints in the patient is thoroughly unscientific, as it fails to differentiate between a large number of possible factors. The projective techniques have not been sufficiently widely applied to determine their adequacy for differentiating the results of therapy, and the problem of developing tests for this particular purpose has not been investigated. This is partly due to the fact that relatively few therapists are sufficiently comfortable in the field of psychotherapy to undertake the very long-term type of problem that such investigations represent. The most hopeful sign for the future is that there is increasing freedom for experimentation, together with increasing knowledge of the principles of psychotherapy.

The problem of estimating the results of psychotherapy is minimally threefold. First, it is necessary to develop a method for determining the degree of maturity of the form or pattern of interpersonal relations or transactions, i.e., of group belongingness. Second, it is necessary to determine the range of change of the situation over which this degree of maturity is stable. Third, such measurements have to be controlled in terms of the status of the subgroups and whole-group at the time the measurements are made.

In summary it may be said that the various psychotherapeutic systems are more similar in their basic principles than they are divergent and this is in keeping with Harry Stack Sullivan's observation[18] that "we are all more simply human than otherwise."

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BRAIN AND BEHAVIOR

W. S. McCulloch

I WANT to make clear two of the notions which Dr. Rioch used very ably in his description of psychotherapy: the notion of information and the notion of give and take between human beings in terms of feed-back. Now, one of the most important things that has happened to my generation is that many of us, who started out in psychology and shifted over to neurophysiology, as time went on were compelled to shift from thinking in neural terms to thinking in terms of energy and in terms of structures in the way our grandfathers had done in physics. But as we were getting acquainted with those disciplines, we found a new discipline arising: namely, communication-engineering, which is a much more radical departure from physics than at first appears.

Let me speak first of the physicist. The physicist deals with a world in which things happen or else do not happen. He observes and he records what happens. As long as he is doing this and formulating theories about it and testing out those theories, he remains a physicist and nothing more. But the moment a physicist tells you, "This instrument is not behaving according to Hoyle," he has ceased to be a physicist and has become a communication-engineer; for he is now speaking of a signal.

I want first, for reasons that will appear later, to speak of the simplest of all possible signals: namely, the kind of signal which either happens or else does not happen—an all-or-none signal. Physically it is some sort of an event propagated somehow through space and time. It is a perfectly good physical affair in the sense that the physicist can find it then and there. It begins somewhere at one time and it ends somewhere else at a later time. But that is not all that it is. It has a second aspect: namely, it is true or else it is false.

By that I do not mean anything very difficult to understand. If a telephone bell rings and you go to the phone, pick it up, and there is no call, it was a false signal. Maybe lightning struck a telephone pole. Maybe insulation burned out somewhere. One produces such false signals whenever, along the line of communication, one introduces a disturbance which is a real, physical affair, but not the one presumably carried over that line. The basic difference between communication-engineering and physics is that in communication-engineering one deals with a signal, an entity which, even in the simplest case, is true or else false, a property which no simply *physical* thing has as a physical *thing*. It is this property, of being true or else false, that in the case of all-or-none signals, makes it possible for us to set up an easy way of keeping track of information, quantitatively.

Let me try to make clear the notion of information as we obtain it from the world. We have receptors, so-called "pick-ups," devices which can initiate impulses in neurons that lead those impulses into the central nervous system. The impulses that come from those receptors are all-or-none in this sense, that whenever a neuron is excited it does all that it can then do. This naturally follows, for a neuron is a distributed telegraphic type of relay, and the size of the signal it believes depends on its sending end, not on the size of the signal it received. When an impulse arrives in the central nervous system, it only signifies that a neuron was adequately stimulated; that is to say, was stimulated in the manner appropriate for the stimulation of that neuron.

If we put an electrode on that neuron and initiate an impulse in it, the impulse is a perfectly good physical impulse, but it is a false signal. If you press on your eye, you will see a light when there need be no light. I mean nothing more when I say "false signal."

Now let us consider a collection of neurons, each of which is to be excited by something in the world outside; but these neurons are not to be so connected that the firing of one in any way influences the firing of any other one. This is to say they do not constitute one system, merely an ensemble, or collection of systems. Let us consider one such neuron first. It has two possible states in any one relay time, which is about a millisecond. It has either one signal or else it has none. Consider two, and there are now four possible states: a signal in the first, but not in the second; a signal in the second, but not in the first; a signal in both; a signal in neither.

Similarly, for three neurons there are eight possible states and for four neurons there are sixteen. In general for N neurons there are 2^N possible states. If, now, the chance that a neuron has an impulse on it at a given time is one-half, the chance that two will have is one-fourth; three, one-eighth; and all N neurons is

$1 \div 2^N = 2^{-N}$. We may define a unit of information as a decision whether or not a given neuron is to have a signal on it. N such decisions specify one and only one out of the 2^N states. Thus our amount of information is simply the logarithm to the base 2 of the reciprocal of the probability of the state.

When light falls on the photoreceptors of the eye, no impulse leaves a ganglion cell unless several receptors attached to it through bipolar cells are simultaneously excited. This decreases the probability of an impulse in the ganglion cell occurring by chance. There are about a hundred million photoreceptors in the eye and only a million ganglion cells. That is a reduction of about one hundred to one between photoreceptors and the channels over which the information is sent to the brain. By demanding coincidence before we pass the signals on to the brain we have insisted that many of our sensory receptors agree in the assertion that there is something there before we bother to pass it on. That is a loss of information. And that information given up is used by us to buy security that we are responding to something which is there weeded out through us to such an extent that our maximum output is about one decision per millisecond when we are lecturing, playing the piano, or whatnot. The maximum amount of information that I can convey is this: one part of one-hundred million of what my eyes can receive. No device that man has ever made corrupts information at such a rate, and none is less apt to go awry, because the brain bases its one decision per millisecond upon such a vast influx of information.

Such is the general nature of our information. If we have a nervous system which is composed of telegraphic relays, neurons, each of which will emit a signal on receipt of a combination of signals provided it is not prevented from doing so by some other signal, and if we have spontaneously acting neurons in its net, we know that a nervous system if properly designed can compute any computable number. The proof of this is Turing's construction of a finite machine that can do these things for similar operations. The question remains, how do we do things that we do do? And that is quite a different question. That requires looking into the nervous system to find out what the mechanism is, because there is always a host of ways in which a finite machine can do any of these things. It only has to be given enough time.

Now, one of the important questions that arises is how one has ideas at all. One looks in vain through the ordinary psychological theories for any conception of a way in which one could perceive a square regardless of its position, its size, and so on. Very often one thinks that he has a general conception only to find on looking into it that he has a term but doesn't have the general conception belonging to the term. He only has some particular. Let me put it this way. One way of having the idea of a dog would be this: if I give you one dog and from that dog you make another dog, and another dog, until eventually you make a particular French poodle, and when you get to that French poodle you stop and say "uh-huh, dog," then all you need for action is your particular idea of the poodle and the word, "dog"; but you don't have the general idea corresponding to the term "dog." You have what is for some the canonical representative of dogs. This is in a sense a way of having a general term for which the idea is not fully general.

But there is another way in which you could truly have the universal or idea "dog." If, when I gave you one dog, you made all dogs and then took the average length, the average width, the average height, the average leggishness, the average hairiness, the average barkiness, the average tail-waggingness, and so on, you would come out with a set of averages each of which in no way depends upon which dog I had given you. For, given any one you would have made all. You would, as we say, have constructed an invariant of the group of dogs. There are some bona fide general ideas that we have which are universal in this sense that they are of all dogs, not just of a representative one: I mean, in which the real idea is dog quite apart from its representation by a French poodle.

I would like to tackle the simplest way in which one can have such a general idea. Let us suppose that you walk into a restaurant where you never were before. You open the door and you smell a smell you *never* smelled before; you shut the door and walk away. So long as you remember that smell you may forget the time and place where you smelled it and still have the idea. You have pulled it out of time. What one needs to get an idea "freed up" from temporal reference, in other words, is a memory of some kind.

The simplest kind of memory can be composed of the same relays which we use for transmitting information: namely, neurons. If we construct a ring of relays and send into that ring a signal patterned after something in the world, say we feed in "di-di-di-da," as long as "di-di-di-da" chases its tail around the ring we have it at all times though we received it only once. Given it at one time, we have made it at all times by simple reverberation. Consequently, it no longer matters when we received it. We know now that there was some time such that at that time, "di-di-di-da."

Now every other variety of memory is only a substitute for reverberation. We may build reverberative out of telegraphic relays; we may build them as acoustic tanks; we may build them in a host of ways. Into any of them we put a signal, let it come around and regenerate it—so that it is sharpened again and again as it goes round and round and round. The difficulty is that such memories require a continuous output of as much energy as is required for transmission of the signals. Consequently, we are in a habit of making marks on a piece of paper or something else of the sort and then looking at them again. In other words, there is a second possibility of a memory: namely, some sort of trace. Apparently we make traces in our brains also—and these traces seem to be divisible into two major groups according to their behavior over long periods of time and according to how they respond to repeated testing.

One type resembles growth with use. It is important in skilled acts which, as we test them and test them and test them, tend to grow worse, but when we let them rest they reappear so much better than before that we seem to learn to swim in the winter and to skate in the summer.

The second is a type of trace which does not ordinarily need any repetition but is repeated by testing. This kind of memory holds snapshots of the world. Although there was a man named Babinski who seems to have been on the trail of it, much earlier Craik, in England, first clearly established that we take these snapshots at a rate of about ten per second. It may be eight per second; it may be twelve per second. But if we try to hurry up this procedure, before we get to twenty per second, it drops back to the half. If we try to slow it down, before we get to five per second, it starts making double-takes.

This became of importance in hooking man to his own electronic devices, particularly radar, where his inability to observe things in the "skip-periods" became crucial. It was then found that you could lock in his visual affairs with auditory signals and push them up or down so that you could get down nearly to five and up practically to twenty. The man who did the first most important work on that problem is John Stroud, a psychologist on the West Coast.

At present I do not want to go into the physiological and anatomical reasons why we should expect such an affair; but I cannot imagine anything except something "quantized" this way that would account for our form vision. To do it not ten times per second but at, say, 1,000 per second requires more brains than could be folded up so as to get it small enough to get it in the head. Let us simply suppose that one takes ten snapshots of the world in a second. And let us be fairly generous, and say that there are about 1,000 binary decisions at the most in any one such snapshot. Now I am going to do a rough piece of arithmetic. Eight per second, 64 seconds per minute, 64 minutes per hour, 16 hours per day, 256 days in the year and 64 years in the life is 2 to the 33rd power, which is 10 to the 10th frames, with 10 to the 3rd items per frame is 10 to the 13th: a rough guess as to the size of human memory.

Heinz von Förster who wrote *Das Gedächtnis* was one of several theoretical physicists who became interested in the size of human memory. Working from the best psychological data available, he came to the conclusion that the mean half life of a trace in human memory is something of the order of half a day. Under those circumstances it is possible for him to make a guess as to the size of human memory by knowing that the memory has to be big enough so that it will never be filled; and at the same time not so big that there will never be much of it used. In other words, if you make traces in such a memory, and they evaporate before you get around to taking them out again, it is no use putting them in. Man has a memory with a half life of half a day. Man has about ten to the seventh, perhaps ten to the eighth, access-channels to put things into it; and each takes a millisecond to get its signal in. This says human memory should be, if you had to design it for such a machine, something of the order of ten to the thirteenth if you're going to be stingy, and ten to the fifteenth if you're going to be generous. Notice this is of about the same order of magnitude that we came to from von Förster's theory of snapshots.

But there's a peculiarity about this memory: namely, that it does not go away asymptotically towards nothing. It goes away towards a residuum which has been variously estimated anywhere from 1 to 12 per cent. How good the evidence is for that amount, I do not know; but let us make it 10 per cent. Now, no number of curves that go away towards zero, if you add them up, will go away towards anything but zero. If the traces in the brain, like most of the things we know about in the world, go away on a thermal basis, then we may guess several things about this memory. But none of them would account for this retention. We would have to suppose that there was some process which re-engraved the traces on the memory. So to speak, all cats die; pretty soon no cats? Yes. But cats have kittens!

We can now estimate the amount of energy that would be required to make such a trace. If it has a half life of half a day, it will have a barrier equivalent to about 1.42 or 1.43 electron volts. To account for retention with memory of a mean half life of half a day, having things that last forever in it to the tune of 10 per cent, would require of the totality of remaking traces about .02 watts. The energy requirement is not great considering that the brain is about a 24-watt organ. In other words, human memory is energetically possible.

There are several things about this kind of memory that are extremely troublesome. In the first place, where is the trace? Is the trace in any one place? Is the trace multiple? Is the trace some peculiar shift in the parameters of the net everywhere? Well, let us look first at what kind of a thing we can have to account for it, if we are going to have to have something of the order of ten to the thirteenth to ten to the fifteenth traces.

You only have something of the order of ten to the tenth neurons in your head. You cannot put one memory in each cell and lock it up there. You haven't got 1,000th of the brains for it. You would have to do it then by some change in the threshold of the cell in such a way that the configuration of endings on it to fire it would be different after, from what it was before the trace was made. Or you have to change the properties of the end-feet: they are numerous enough. The required number of combinations you can make by, so to speak, wiring up inside the neuron so that it will fire for one combination and not for another is theoretically quite possible.

Protein molecules have a half life of the general order of half a day. Protein molecules have the trick of generating others in their own image. That is how your genes made you in the first place. So that it is quite conceivable that snapshot memory is going to turn out to be a matter of protein chemistry where the crucial item is a structural protein, linked perhaps to a carbohydrate, to a phospholipid, or to nucleic acid. That is what one would expect.

Next, in how many places must it be in the nervous system if it is going to persist? It can't be all in one place or a single lesion would knock it out in a highly specific fashion, and that is not the general story. The most likely place to look is probably somewhere in one of the great masses of small cells which conceivably can be linked up to form some facultative paths; those facultative paths might be something which was temporarily active over quite a long time until its properties became altered by that activity.

If you were going to try to find such a thing in the spinal cord, the only place to look for it is the *substantia gelatinosa* of Rolando. If you are going to find it in the midbrain, it's got to be in the reticular formation. And it might, of course, be in the bark of the brain, or cortex. The latter does not seem to me to be as likely as those other places. As Dr. Rioch could probably tell you much better than I can, if you leave the midbrain intact and take off the brain above it, a cat still learns. The loss of the cortex seems rather to knock out the possibility of having certain kinds of ideas than to knock out memory itself. It may be a process distributed throughout the nervous system, but if I had to guess its whereabouts, I would put my money on the midbrain.

There is one more point I want to make. We can have ideas retained in memory perhaps by reverberation and so abstracted from their temporal reference. In other words, have them apart from reference to the time at which they occurred. Also, we have devices like those that turn our eyes mediated over a circuit running over the superior colliculus, that will center a form to be seen before we have to go to work on it to tell what shape it is. And one's vision for shape is amazingly poor with objects which are not centered, but this device, having centered it, makes it a matter of no moment where it first appeared.

There are nice problems that arise such as how one can hear a chord regardless of the pitch and know that it is a major third or diminished seventh, or whatnot. One obviously cannot twist his ears so as to change the key. If it is given in C, it is given in C, yet one can still recognize the major third or the diminished seventh.

Imagine for yourself layer upon layer of mosaics of relays. Let the output from those mosaics descend straight down. Send your information up channels that slant upward through it. Set the threshold of each of those relays at a value so high that it requires energizing of that whole layer and a signal of a particular slanting fiber to fire any one relay. Now when you energize the lowest layer, your signal will go out practically where it came in, and as you energize higher and higher layers it will step off in the direction of the slope of the ascending fibers. In other words, the first time it will go here, the second time one step that way from here, the third time two steps, and so on.

There is reason to believe that the auditory cortex has a structure somewhat of this kind. We want at the present time to have new histological sections of it made along what is practically the axis of pitch. The interesting thing is that pitch is projected to the cortex so that octaves span about equal distances on the bark of the brain. Under those circumstances, if you bring your information through those hundreds of layers of relays that constitute the bark of the brain and send it down vertically, you will expect that you have taken whatever you got by way of an input and, in effect, translated it along the axis before you let it out again. And from this you are in the position to do what you did in the case of making the average dog. You will have been able to take something given at one pitch, make it at all pitches, and compute your averages. You will have translated each note in key without changing the interval. Mechanisms of this kind may account for our general ideas.

I have given what I hope is an oversimplified statement and probably it will not turn out to be true just as I have given it to you. The difficulty in guessing at the central nervous system functions is always that any function given simultaneously in space in a set of relay nets can be converted into one given sequentially over a single neuron. You will need, then, as many relay times as you need relays in space. The problem that lies before us who work on the physiology of the brain is to propose for ourselves mechanistic hypotheses as to its working. These must be such that they can be checked anatomically, to make sure that the nervous system is so wired, and physiologically to see that it does so operate. In that we will expect aid. At the present time our physiological knowledge is ahead of our anatomical knowledge and several of the best neurophysiologists, like Lörente de Nó, are going back to discover the necessary anatomy.

The brain as involved in behavior is for the psychologist that which is in the black box between an input and an output. He is welcome to make any kind of assumption concerning its content that he has a mind to, provided it will give him that output for that input. The difficulty is that when he seeks for a model to represent the inside of that box, it is extremely difficult for him to invent one that will give him the required output for that input. And it is Rioch's delightful proposal that the proper model for the action of the brain in behavior is the action of the brain! That's only hard on us, the physiologists, who have to find out how it works.

PRINCIPLES AND PROBLEMS OF THEORY CONSTRUCTION IN PSYCHOLOGY

HERBERT FEIGL

AS A logician and methodologist of science, not directly engaged in specific empirical or theoretical research, I can contribute to this symposium only in the role of an interested spectator. It was with this understanding that I accepted the very gratifying invitation to the present symposium. Although an outsider, or perhaps just because of this role, I may be able to provide a critical perspective of some problems of theory construction, which those actively immersed in constructive work might find useful for purposes of clarification and broader methodological orientation. It is precisely from a background in the history and in the comparative methodology of recent and current developments in the sciences that we may justifiably hope to obtain some helpful suggestions.

Let me begin, however, with a few remarks on the *psychology* of the theorizing psychologists. There are bits and spurts of more or less successful theorizing in limited regions of psychology. The fields of perception, learning, motivation, personality structure, etc., have each been organized by fruitful guiding ideas and special techniques of investigation. Encouraged by these conquests, some psychologists in recent decades have searched for a higher integration. The proud Newtonian synthesis and everything that developed in the theories of the physical sciences since the seventeenth century has inspired many attempts at emulation in psychology and the social sciences. But only scant success has rewarded the efforts of the theorizers. They have attempted to discover a unitary conceptual system which would encompass all the special fields of psychology and thus overcome the extant regionalism. These "Unitarians" (if I may briefly call them thus) have a very high level of aspiration. But its discrepancy with actual achievement when beheld in the light of sober critical reflection has frequently produced a sense of frustration or of inferiority. As psychologists well know, frustration may lead to regression; and the regression may result in aggression or in an escape into irreality. Aggression may sadistically be directed against outgroups, scapegoats, or strawmen. It may also masochistically revert to the proponent himself and result in a radical constriction of his own ambitions. I am sure you know whereof I speak. As regards the overcompensations by way of escape into irreality, I might quote what was written not so long ago by an astute philosopher of science, C. D. Broad of Trinity College, Cambridge: "Poor dear Psychology, of course, has never got far beyond the stage of mediaeval physics, except in its statistical developments, where the labours of the mathematicians have enabled it to spin out the correlations of trivialities into endless refinements. For the rest it is only too obvious that, up to the present, a great deal of Psychology consists mainly of muddle, twaddle, and quacksalving, trying to impose itself as a *science* by the elaborateness of its terminology and the confidence of its assertions."[10]

Let me reassure you immediately that I do not for a moment share Broad's disdainful attitude. I can concur with some of his strictures only if the word "science" is replaced by "comprehensive theory." There is no question in my mind that the psychological theorizing, especially of the last four or five decades, has produced very valuable results and even more valuable suggestions for the advancement of further research, both theoretical and experimental. The problems and principles that I wish to discuss concern the logical structure, the basic concepts, the scopes and limits of the more successful psychological theories.

To begin with, it may be useful to review very rapidly a few elementary notions. A set of propositions may be called a "theory" for various reasons. It may be because these propositions are highly inferential—either because of the sweep of their generalizations or because of the remoteness of the concepts from those of direct observation. That is, we tend to regard assumptions as theoretical if they are only very incompletely or very indirectly confirmed. This customary terminology does not altogether recommend itself for the simple reason that a well confirmed set of propositions could then not be called a theory. Yet, such is the ambiguity of our terms, that by a "theory" in the empirical sciences we may mean anything from a style or jargon of mere descriptions, from a mere classification, inventory, or typology to a full fledged hypothetico-deductive system; from a bold guess or a suggestive working hypothesis, a program of research, to an elaborate model in either analogical or purely abstract mathematical terms.

In order to provide for a terminology which will not constantly involve us in a tangle of confusions, I propose to define a "theory" as a set of assumptions from which can be derived by purely logico-mathematical procedures, a larger set of empirical laws. The theory thereby furnishes an explanation of these empirical laws and unifies the originally relatively heterogeneous areas of subject matter characterized by those empirical laws. Even though it must be admitted that there is no sharp line of demarcation (except a purely arbitrary one) between theoretical assumptions and empirical laws, the distinction, at least in the sense of a gradation, is illuminating from a methodological point of view.

One more terminological suggestion may help: Let us speak of scientific explanation wherever more specific or more descriptive statements are derived from more general or more hypothetical assumptions. The derivations of special cases from empirical laws, or of more restricted empirical laws from a set of broader laws may justly be called explanations even if they have the taint of frivolity. Markedly theoretical explanations can always be distinguished as "high level" or in any case "higher level" explanations.[27] I suggest this sort of vertical representation because of the customary manner of writing premises above the conclusion of deductive inferences. It also conforms with the familiar phrase that designates very abstract and comprehensive premises as "highly theoretical." But to forestall confusion it should be noted that it is usually on the lower levels of explanation that the sciences deal with objects of "higher complexity."

Several levels of explanation in psychology stand out and will now be conveniently arranged and illustrated. The statistical-ecological type of approach in the field of perception as pursued and advocated most prominently by Egon Brunswik[12, 13, 15] is a good example of the search for probability connections on the level of empirical law. Through the sampling of situations in which the perceiving organism attains certain adaptations by means of the cues contained in, or resulting from, the pattern of distal stimulus objects, Brunswik keeps his investigations as close to actual life situations as possible. He stresses this "representative" character of his experimental design and contrasts it with the artificially dissecting and abstractive approaches of both classical psychophysics and the "molecular" analyses of modern psychophysiology. A methodologically analogous procedure in the field of personality psychology may be seen in the case-historical and statistical studies of symptoms and syndromes, based either on clinical observation or psychometric tests. Raymond Cattell's studies of traits and trait clusters are a good example of this close-to-life type of research. The trait names themselves are taken from the psychological and characterological vocabulary of common language, and individuals are assigned positions in rank-orders or topological hyper-spaces in accordance with the results of close-acquaintance ratings or "social perception." The kind of explanation achieved here is of the low level type. Individual facts (events, presence or absence of traits, symptoms, etc.) are subsumed under empirical laws, and these laws are, inevitably of statistical nature.

By way of a rough analogy, the procedure on this level of analysis might be likened to studies of the performance of automobiles by a purely statistical approach. Automobiles of different makes (species), of different ages (maturation), of different previous treatment (learning), manifest different features such as maximum speeds, pickup, gas and oil economy, durability, noise, mountain-climbing capacity, etc. (achievement variables). A detailed study of the intercorrelations between these trait variables can furnish useful information from the point of view of consumers' research. Such information may be made more reliable by the application of the sort of tests that are administered by means of special instruments designed to determine parameters like the efficiency of an engine. The validity of such tests could be scrutinized by matching their results statistically against performance on the road. The reliability of the tests is subject to the usual questions. For example, if the test was administered on an especially cold day, the result may not be as dependable as under more normal conditions. If this analogy involving inorganic machines appears too far fetched, it could be replaced by others taken from biological or medical areas. Achievement variables, for example, in athletic performances are readily definable and studies of their intercorrelations, factor-analyses, etc., have been applied in this field. Special medical tests like those concerning the rate of basal metabolism would be analogous to psychometric tests.

The limitations of this type of approach in psychology are obvious. While the procedure is thoroughly empirical, i.e., "operational" in the narrower sense of this word, the molar and distal type of analysis yields at best a set of empirical laws, always of statistical character, often with only rather low degrees of correlation. It is, in my opinion, a very detrimental fallacy, if psychologists try to reconcile themselves to this state of affairs by reference to the possibly statistical nature of even the basic physical laws as formulated in contemporary atomic and quantum physics. It is a very far cry from the macro-phenomena of behavior to the micro-processes of today's "rock-bottom" physics. I do not mean to assert that the quantum indeterminacy with which we have to reckon in the interaction of the basic particles with each other and with electromagnetic radiation might not show up in the more delicate and minute aspects of neural discharge or conduction. Nor would one wish to deny that these basic indeterminacies might, through various amplification processes characteristic of much that occurs in organisms, be carried to the surface of gross and overt behavior. Speculations of the sort presented by Schrödinger[52] regarding the role of quantum transitions in gene mutations furnish a plausible analogy. But aside from these considerations which suggest the presence of "divergent"[43] processes in all behavior, there are even stronger reasons to assume that the vast majority of those basic chance-events does not in any way become manifest on the macro-surface. The "law of large numbers" here, as well as on the more familiar level of statistical thermodynamics, indicates clearly that there is an overwhelmingly great probability for "convergent" processes, as evidenced in the practical irreversibility of almost all macro-processes. Even if we operate on a level, which from the micropoint of view must be regarded as a statistical "envelope," there are plenty of reliable functional relations of a more nearly deterministic sort between the molar variables. My point is simply that it is incongruous, immaterial, and unjustifiable to indulge in a "sour grapes" excuse and thus to declare prematurely failure in the search for deterministic or near-deterministic laws. Science has always been, and presumably will always be, the search for laws that permit prediction with maximal probability and exactitude. Explanation, prediction, and control remain the supreme aims of the pure and the applied sciences. Wherever possible, scientists try to improve the reliability of their knowledge of what happens under what conditions.

Experimental biology and psychology have, of course, never been seriously disturbed or inhibited by scruples regarding determinism. Indeed, the deliberately ecological-statistical type of approach originated primarily from the practical needs for, and the demonstrable feasibility of, an analysis of life and behavior in their natural settings. The typical experimental approach of the "artificial" and "dissecting" type preceded it in psychology, starting with Weber, Fechner, and Wundt and is still with us in the work of Hull, Tolman, Krech, Lewin, Skinner, Münzinger, Miller, Dollard, Mowrer, Masserman—to mention just a few of the more influential researchers. The aim of all the ingenious experiments is obviously to catch hold of manipulable or controllable variables in terms of which the observable, macro-behavior of organisms can be most effectively formulated. The functional relations of these variables constitute the laws of our science. In order to focus attention upon this level of analysis and explanation, it is necessary to disregard whatever concepts and hypotheses are introduced on higher levels of explanation. We are now concerned with the concepts and laws that formulate what has aptly been called the behavior of the "empty organism." This level of approach is perhaps best exemplified by B. F. Skinner's outlook. (To use our automobile simile once more, we are trying experimentally to determine, e.g., the rate of the acceleration of a car as dependent upon the depression of the accelerator pedal, the octane content of the gasoline, the incline of the road, etc. But we have not permitted ourselves to peer into the machinery.) The research of Hull and his school, by and large, operates on this molar level. Neurophysiological concepts are introduced only in a marginal manner and function more in the role of promissory notes for a higher level of explanation to be attained in the future.

According to Spence[57] "the only meaning that these theoretical intervening constructs have *at the present time* is provided by the equations that relate them to the known experimental variables—the environmental measurements on the one hand and the behavior measures on the other. Such equations constitute the definitions of these terms." This statement by an important member of Hull's school of thought indicates clearly that he interprets the concepts of behavior theory (their neurophysiological phrasings to the contrary notwithstanding) essentially as intervening variables and not as hypothetical constructs in the sense of the distinction drawn by MacCorquodale and Meehl.[44] In effect, then, Hullian behavior theory is essentially molar in its level of investigation. It is, however, well worth noting that the term "molar" is used here only in one or two of the seven meanings enumerated and discussed by Littman and Rosen.[41] Connected with the still relatively molar character of this sort of analysis is its preoccupation with proximal stimuli and responses. The independent as well as the dependent variables are essentially peripheral or near-peripheral.

It is this level of analysis and explanation that is generally regarded as the beginning of theory. But, as I indicated before, there are no sharp lines that could be drawn between explanations on the basis of experimental laws and explanations on the basis of more comprehensive or existential assumptions. The deductive fertility that we count among the desirable features of theoretical systems may be present in varying degrees in either case. As I see it, Hull's hypothetico-deductive procedure, both in the *Mathematical Theory of Rote Learning*[32] and in the later *Principles of Behavior*[33] yields—*more geometrico*-explanations not only of individual events, but more notably of special regularities formulated in empirical laws. These theorems are derived from postulates which employ quantitatively defined intervening variables (such as stimulus trace, habit strength, excitatory and inhibitory potentials, effective excitatory potentials, etc.). The question of the factual adequacy of the special content of the behavior theories of Hull or others is, of course, of primary importance *within* psychology. We can afford to leave this question in the competent hands of the experimenters. What interests us from the methodological point of view is the fact that here we have before us a concrete and detailed attempt at theorizing. No matter how much it may have to be revised in the future, it demonstrates more clearly and more specifically the type of hypothetico-deductive procedures that can be pursued and the type of results that can be achieved on the molar level. It is encouraging to see that Hull's work fulfills in a significant manner what in the (in many other respects) quite divergent psychological outlook of Tolman's theorizing was conceived as a brilliant, but on the whole only sketchily outlined program.

It is instructive to compare this kind of molar behavior theory with some typical macro-theories in physics. The parallels that suggest themselves are those with classical thermodynamics or with macro-electromagnetic theory. Classical thermodynamics, in contradistinction to molecular or statistical thermodynamics (not to mention the still more recent quantum-theoretical developments), is typically molar in its level of analysis in that its concepts are either experimentally fairly directly accessible variables such as temperature, pressure, volume, etc., or are capable of explicit definition in terms of these observables. Thus, the concepts of energy and entropy which figure prominently in the basic postulates of thermodynamics might be regarded as somewhat analogous to intervening variables in the sense of Tolman and Hull. A number of important aspects of countless types of physical and chemical processes have been most successfully explained on the basis of the principles of thermodynamics. The laws of thermodynamics give highly significant information on which sort of processes can, and which sort cannot, occur in certain physical or physicochemical situations.

The precise prediction of individual events, however, depends upon the knowledge of certain characteristic constants and parameters. In the application of classical thermodynamics to specific problems one must know something not only about the initial conditions as expressed by assigning concrete values to independent variables, but one must also know the values of certain characteristic constants such as the specific heat of the substances under consideration. Similarly, in the study of the electric or magnetic behavior of various substances one must know such constants as their electrostatic capacity, conductivity, dielectric constants, magnetic permeability, and susceptibility. The macro-approach of classical thermodynamics and of electromagnetics must take this sort of information for granted as it is furnished by special measurements. Analogously, in macro-behavior theory there are constants and parameters whose values must be ascertained in order to proceed to specific explanations and predictions. Hull's "M," i.e., the limit of habit strength, may serve as an instructive parallel. This parameter varies of course for different species of organisms, but it varies also with the age (maturation) of the individual organisms and is, finally, also subject to individual differences. Behavior theory on the macro-level cannot account for these facts any better than macro-physics can account for the characteristic constants of various substances.

It is here where we find the inevitable limitations of the molar level of analysis. In keeping with C. D. Broad's sarcastic remark one might say that physics as well as psychology cannot get away from the mediaeval ideas of powers, capacities, or faculties as long as they remain on the macro-level. It will be worth while to ask just what is so objectionable in the use of these mediaeval concepts. The answer, it seems to me, is threefold: First, there is the temptation to view capacity or disposition concepts as purely qualitative instead of quantifiable—which later most of them turn out to be when subjected to modern experimental and mensurational procedures. Further, in the context of these procedures a second objectionable feature usually disappears, namely, the (alleged) absolute character of those powers. Soon enough most, if not all of them, reveal themselves as functionally dependent upon other variables. The third objection which is traditionally the strongest, has, I think, ever since the instinct controversy of some twenty-five years ago been removed by attention to the proper level of explanation. "Powers" or "dispositions" are to be viewed as shorthand for laws that tell us what results are expected to arise under certain test conditions. As long as we do not fool ourselves into thinking that dispositional concepts explain the very law by which they are expressly (or tacitly) defined, they can still serve as helpful tools in the explanation of specific events. In other words, and in terms of the time-honored example, reference to the soporific power of opium does not explain the empirical regularity for which it is merely a shorthand formulation. But it does account for each individual case in which opium is found to have its well-known effect. This is of course what I called "low level explanation." Higher level explanation would have to enter into the physiological-chemical effects of alkaloids upon the nervous system.

It is interesting to note how modern empiricists—who are generally expected radically to oppose the Aristotelian and scholastic tradition—have come to rehabilitate the dispositional concepts. I am not only referring to the prominent role of intervening variables, such as Tolman's "behavior readinesses" or Hull's "habit strength," but also to the work of such philosophers as Carnap and Ryle. Already in his early formulations of a "logical behaviorism" or of "psychology in physical language," Carnap[16] attempted a translation of psychological concepts with typically mentalistic flavor into objective behavioral concepts. The logical form of this translation was later made more explicit by Carnap in his account of conditional definitions or reduction sentences.[17] In the light of more recent work in logical analysis it seems likely that this specific form of reconstruction of conditional definitions may have to be modified—perhaps in the direction modal (causal) implication or else of probability relations (degree of confirmation). In any case, applications of Carnap's original analysis to specific psychological concepts such as motivation, expectation, as well as to the general problem of the meaning of intervening variables may be found in articles by Koch,[37] Tolman-Ritchie-Kalish,[63] and MacCorquodale and Meehl.[44] In a recent book[51] the philosopher Gilbert Ryle of Oxford shows with great penetration and extremely subtle awareness of linguistic peculiarities how to formulate many recalcitrant mentalistic concepts in terms of behavior dispositions. Even if this sort of thought from England comes so late as to be almost anachronistic, it is nevertheless delightful that it could come at all from a school of thought closely related to that of C. D. Broad who twenty-five years ago said of behaviorism that it belongs to the kind of theories which are "so preposterously silly that only very learned men could have thought of them."[9]

Returning to intervening variables as dispositional concepts, several points should be made explicit. Dispositions as we understand them in common parlance, in the Aristotelian tradition, as well as in modern scientific concept formation, are in any case properties, traits, features of things or systems. Inorganic substances, organisms, persons, are the sort of objects to which we ascribe these features in subject-predicate sentences. Wherever science can get beyond this relatively crude level of categorization in terms of substances, causes, and effects, and manages to replace them by a language of events, field conditions, and functional relations, dispositional properties are no longer indispensable. But on the way to this final stage, which is realized fully perhaps only in modern field physics, there is usually first a gradual reduction in the number of the (on the given level) basic dispositional properties. This sort of reduction is obvious in the progress from nineteenth-century chemistry to the atomic and quantum theory of the chemical bond. A great variety of the properties of erstwhile irreducible chemical elements and compounds can be explained in terms of the basic dispositional properties of the fundamental particles. A parallel case may be seen in the transition from phenotypical to genotypical studies in biology, or from surface traits to source traits or factors in personality psychology. This is at least an important part of what Kurt Lewin[38, 39] had in mind when, following Ernst Cassirer, he emphasized the difference of Galilean from Aristotelian ways of thinking in science. Since, however, Galileo's physics, and most science until the late nineteenth century, was still replete with unreduced dispositional concepts, it might be better to contrast the Aristotelian with the Maxwell-Einsteinian type of concept formation.

The main point to be noted in the successive reduction and elimination of dispositional concepts is that it (usually) involves a distinct type of theorizing. For want of a better term I shall call it the method of "existential hypotheses"[22, 23] or, following MacCorquodale and Meehl, the method of "hypothetical constructs." Briefly and roughly, an existential hypothesis consists in the identification of a concept thus far given only by conditional definition, i.e., an intervening variable (in the narrower sense), with a concept of genuinely hypothetical character introduced on the basis of some new and heterogeneous area of evidence. In physics, chemistry, and biology there are, of course, countless illustrations of this sort of theorizing. For example, the elasticity of a rubber band may be first introduced as an intervening variable, merely shorthand for the qualitative or quantitative aspects of the stretching and the subsequent contraction. But, from the point of view of a molecular approach, aided by the evidence from chemistry and X-ray spectroscopy a micro-explanation for this sort of behavior has been achieved. Similarly, the quantum theory of ferromagnetism provides the hypothetical constructs which help to account for the macro-phenomena of magnetization. The

Mendelian conceptions of hereditary factors are increasingly filled out by new hypothetical constructs of the theory of genes. The classical example from physics is, of course, the identification of the macro-concept of temperature with the concept of a class (or disjunction) of micro-states of the molecules, in terms of their average kinetic energies. The general methodology of these procedures should be clear enough: At the price of existential hypotheses we achieve a reduction (often a very considerable one!) of the hitherto unreduced dispositional concepts or intervening variables.

It may be useful to illustrate more specifically the methodological parallel between the respective macro and micro-explanations of physics and psychology. Although my example is deliberately chosen to display also a certain analogy in regard to subject matter, I do not for a moment wish to suggest any puerile speculations as far as content is concerned. Consider the simple processes of learning in Pavlovian or Skinnerian conditioning and compare them with the process of the magnetization of an iron bar. There are certain concepts that are needed for the formulation of these cumulative processes on either side. There are, first, fundamental dispositions such as conditionability (trainability), and corresponding to it magnetizability. Vertebrates generally show this basic trait in varying degrees, invertebrates either lack it or possess it only to a very slight extent. Iron, cobalt, nickel, and certain alloys show high albeit varying degrees of magnetizability. Copper, aluminum, or carbon lack it entirely. Let us next consider, the cases of the albino rat and that of iron in the respective processes of conditioning and of magnetization. Roughly speaking, we have here certain regularities that can be expressed with the help of such concepts as number of reinforcements, and respectively, number of exposures to a magnetic field. The degree of modification thus attained may be expressed in terms of habit strength (or Skinner's "reserve") and in terms of pole strength (magnetic intensity) in the parallel case. We may next study the processes of inhibition and of experimental extinction, and parallel to them such processes of demagnetization as through increase of temperature or sharp mechanical impact. It is obvious how this yarn could be spun out to greater length. But I don't wish to bore you. My point is obvious: Given the empirical laws covering the processes on either side, we can derive further empirical laws that enable us to predict what happens in all sorts of more complex situations, such as alternate or combined processes of reinforcement and extinction. Only molar concepts are used. Maximal magnetic susceptibility of a material corresponds to maximal habit strength, but the values of these parameters and their relation to the (negatively accelerated) growth curves in either case remain—on this level of analysis—unexplained. We deal with quantified dispositional concepts which are irreducible and simply to be accepted in the molar approach.

Let me point out in greater detail some of the inherent limitations of the molar level of theory construction. I am utilizing here an argument developed in our joint seminar on Philosophical Problems of Psychology, by Paul Meehl to whom I am very much indebted for a great deal of clarification of these and other methodological issues. Such concepts as *drive, habit strength, expectancy* in systems of the Hull or Tolman type are introduced through their logical "antecedent and consequent anchorings" in the independent and the dependent variables (the observable conditions and responses) of behavior. But matters are not as simple as all that. All available behavioral indicators of these intervening variables are influenceable by more than one kind of inner determinant. If a rat does not press the lever, he may have been extinguished: he may not be hungry; or he may be afraid. This fact of multivariate determination may seem trivial but it casts serious doubt on a simple-minded, ultra-reductive operationism. If such concepts as *hunger* or *habit strength* are defined strictly and exclusively in terms of their respective indicators, it must be understood that the antecedent conditions are such that all the other internal variables which can influence the indicators are held at constant values. How can we make sure of this? The answer would seem to be by utilizing further reductions (i.e., conditional definitions) for those interfering variables. But here is the rub: the functional relations of the interfering variables to their indicators are often unknown, or only so poorly known as to be of no avail. In speaking of these relations as functional, I am stressing that this is a matter of factual knowledge rather than of mere definition or conventional stipulation. The expansion of the mercury column in the ordinary thermometer may of course arbitrarily be taken to *define* the temperature of the body subjected to thermometrical measurement. But it is well known that other thermometric substances such as alcohol, air, etc., yield quantitatively different results. In the end, physicists had to settle on a more involved definition of temperature (the thermodynamical concept of the Kelvin scale) because only in terms of this concept do the laws of energetics assume their simplest and most universal form. Here is one more warning against the naiveté of a conventionalistic operationism. The meaning of a concept is quite generally *not* identical with the operations performed or operation results obtained in the usual mensurational procedures. Returning to operational behavior studies we may say that any "arbitrary" definition of the obfuscating or nuisance variables is bound to be unsatisfactory in that it will not generally help towards a good estimate of these variables. This will render the antecedent anchoring of the original concept (habit strength, drive, etc.) seriously defective if not altogether worthless. By experimentally holding the conventionally chosen observable indicators constant, we shall then not manage in fact to hold the nuisance variable constant. Hence the indicator variable proposed for our concept will be afflicted with a variation actually due to the nuisance variable.

To illustrate more specifically: The strength of animal and human responses depends (in part) in some as yet unknown way upon anxiety. At the present stage of research we do not even know under what conditions a rat's fear will tend to decrease and under what conditions it will tend to increase the response strength. Moreover, we do not know all the conditions that produce anxiety; we are in the dark as to the relative efficacy; we have scarcely any reliable indications as to rate of spontaneous decay of anxiety, nor of its stimulus generalization curve, etc. The concept of anxiety is thus not securely anchored on either the antecedent or the consequent side. Most talk about anxiety therefore amounts to scarcely anything more than outlining programs of research or of conceptual clarification. Now, if an intervening variable like habit strength is to be anchored to response strength, a *ceteris paribus* clause is, as in all such definition, indispensable. But this proviso demands here either to exclude the anxiety factor or to keep it constant by special controls. But how can this be done? We do not know all the indicators of anxiety and even the ones that are known give us no quantitative, and certainly no reliable, estimates for it.

There are, of course, several gross symptoms of high degrees of anxiety. Lower degrees may be discerned by a more sensitive indicator such as lever pressing. But is there any assurance that still lesser degrees are not present and operating as nuisance variables to deflect choices in a T-maze study of latent learning? Is there any experimenter who has used forced trials without wondering what he was doing to the anxiety level? Are we building on shifting sands? The only thing psychologists can do in molar behavior studies is to steer a middle course between recklessly issuing promissory notes and overcautiously but unfruitfully restricting themselves to purely operational procedures. They need not apologize for being unable to provide strict operational definitions for every concept used. Molar behaviorism cannot avoid these ambiguities and indeterminacies. A strict one-to-one correspondence of indicator variables and intervening variables can be achieved by arbitrary definition—but the severely limited success of the prediction and explanation of behavior on this basis shows convincingly that we must search by whatever means available for a more complete frame of concepts and laws. It stands to reason that this can only be achieved through the method of existential assumptions, i.e., through the introduction of hypothetical constructs.

If space permitted it would be instructive to review here in some detail the story of the positivistic and operationistic battle against existential hypotheses. There is a curious parallel between the opposition offered by Mach and Ostwald to the atomic theory in chemistry and physics and that of some psychologists against neurophysiological explanations of behavior.

A reading of the last chapter of B. F. Skinner's book on *The Behavior of Organisms*[53] shows this sort of resistance against existential and micro-hypotheses. Of course, it is one thing to say that the level of psychological research is by definition to be limited to molar behavior. It is also quite reasonable to say that before ascending to a further level of explanation one must first accumulate a rich collection of empirical laws on the lower level, if for no other reason than that only with respect to these empirical laws alternative micro-theories could possibly be tested and appraised. But it is another thing to say that no illumination whatever can come to our understanding of the laws of behavior from neurophysiology. Kurt Lewin, who had his own ideas on the nature of psychological theorizing, never went quite so far as this. But I remember that in more than one conversation he tried to convince me that the time for neurophysiological explanations would not come for a thousand years!

Permit me a more personal remark in this connection. I have been brought up intellectually in the atmosphere of the logical positivism of the Vienna Circle; I associated for a year with Professor Bridgman at Harvard and learned a great deal about Operationism from him as well as about operational behaviorism from my good friend B. F. Skinner. I have had the benefit of many discussions with Professors S. S. Stevens and E. C. Tolman. I have on many occasions spoken and written in behalf of Logical Empiricism and Operationism. But I have, ever since my Vienna days of more than twenty years ago, consistently opposed the all too restrictive tendencies of some of my friends and colleagues in the positivist and operationist movements.

The intellectual asceticism of the positivists has, of course, been eminently helpful in the critique of transcendent metaphysics. There is apparently always a danger that even scientists, in their attempts to safeguard an originally plausible and personally satisfying hypothesis, make that hypothesis by peculiar reformulations (of which they are usually unaware) proof against disproof. In such cases the positivistic critique is helpful in that it points out that hypotheses which are in principle irrefutable cannot in any way be confirmed either, and thus stand revealed as pseudo-assertions. They may masquerade as scientific propositions, but closer analysis shows that possibly some pictorial or emotional appeal was here mistaken for genuinely factual meaning. The fate of the ether hypothesis is a typical case in point in physics. Freud's doctrine of the death instinct is perhaps a good example from psychology. That it "can happen here" may not without some irony be indicated by the fact that even staunch operationists like Hull avail themselves of hypotheses which in the end may have to be rendered strictly unconfirmable. I have in mind his tendency to safeguard his peripheralist-behaviorist approach by the tenacious assumption of minute fractional anticipatory goal responses (his "rg"). Why should Hull and his followers indulge in this sort of metaphysics when otherwise they are not as opposed as, e.g., Skinner, to centralist or neurophysiological hypotheses? An rg that becomes increasingly minute and recedes more and more from the peripheral to the central regions of the organism is certainly a peculiar kind of response. Baffled as everyone must be at the present stage of the latent learning experiments, would it not be wiser to delay theoretical explanation until the facts themselves are less obscure?

As long as the positivistic and operationist critique insists on confirmability, no matter how incomplete or indirect, as a necessary condition for scientific significance of hypotheses, there should be no objection to it. The warning to keep our feet on the ground, or, if I may change the metaphor, to anchor our concepts and constructs in the data of observation, should in this age of modern science be so banal and obvious as to be superfluous, if it were not for the ever-present human penchant for verbal magic and verbal sedatives.

Unfortunately, and again because of all too human inclinations, positivism has only too frequently become a doctrine of arbitrary, sterile constriction. In other words, it has tended to become a negativism or prohibitionism. Perhaps you have heard the phrase "Dustbowl Empiricism." Comte, Mach, Ostwald, Pearson, and many others among the earlier and later positivists and operationists have at times thrown out the baby of fruitful hypothetical constructs with the bathwater of transcendent speculations. In the name of a misunderstood principle of parsimony (cf. L. W. Beck, 1) they have relegated to the limbo of metaphysics concepts and hypotheses which were perfectly good physics, biology, or psychology. I am pleading, as usual, for the avoidance of the reductive fallacies of the nothing-but-school-of-thought *as well as* of the seductive fallacies of the something-more-thinkers. Among the physicists it is above all Einstein[21] who has consistently maintained an impressively sound and fruitful middle of the road position in these methodological issues. Einstein has repeatedly stressed that the formation of scientific theories is a matter of free construction, a matter of casting a net of concepts and laws, tied only in a relatively small number of points to the ground of experience. But Einstein has been equally insistent upon the trial-and-error character of theorizing in his emphasis upon the data of observation as the ineluctable criteria for the confirmation or disconfirmation of any theoretical system. In his recent book on the *Nature of Physical Reality*[45] H. Margenau has stated much more elaborately, and I think helpfully, the various considerations and requirements for successful theory building.

Modern progressive theory construction has successfully dissented from ultra-positivistic precepts in safeguarding its freedom in the adoption of existential hypotheses. It thus rejects any possible confusion of inductive simplicity with the criterion of meaningfulness. The hypothetical constructs of modern physics, despite their remoteness from the observational data, are well connected with these data in the elaborate network of lawful relationships. Wherever doubts may arise, as on the frontier of advancing theory construction (as in the case of the neutrino, the meson, etc.), the physicists themselves are anxious to discover new connections so that the concept soon becomes more definitely related to other points of the network and thus more fully and more variously to the data of experience. Only if the concept is completely isolated, unconnected, i.e., altogether outside the network, then it must be rejected as nonscientific or metaphysical. But if it is only poorly connected, possibly only through one postulated nomothetic relation, then it is looked at with suspicion from the point of view of the principle of inductive simplicity. The scientist then asks in effect: "Is this trip really necessary?" But wherever we can, to put it colloquially, "get a line" on a concept from various areas of evidence, wherever there is, so to speak, genuine triangulation in the space of concepts, there we have not only a meaningful but possibly also a genuinely fruitful concept.

The case of psychology is especially fortunate. Positivism and Operationism of the detrimental, restrictive sort was here never even as faintly justifiable as in the case of the much bolder hypotheses of physics. After all, it was obvious—indeed, it must have been obvious ever since the beginning of science—that overt behavior is causally dependent to a very large extent upon what goes on inside the organism, and that the anatomical and physiological study of this inside cannot fail to illuminate the regularities found to prevail between stimuli and responses. The rise of psychosomatic medicine and of cybernetics are sufficient proof of the fruitfulness of this expansion of concepts and explanations. Ultra-operationism and positivism are doubly anachronistic in the psychology of our day. These restrictive doctrines have been obsolete in physics for a long time and must be recognized as refuted in psychology by the progress of the more recent neurophysiological research.

Let me now put my argument less propagandistically, but more systematically. There are two large classes of existential hypotheses. Hypotheses of the first type introduce, on the basis of indirect evidence, entities which are, as it were, merely "more of the same sort of thing" already acknowledged in science. When Leverrier and Adams inferred the existence of a further planet beyond the orbit of Uranus, it was essentially the same sort of thing already known from the cases of Mercury, Venus, Earth, Mars, Jupiter, and Saturn. When a neurologist infers the presence of a tumor in a patient's brain, he also is dealing with something of the sort known from previous observations. But it is entirely different with a second type of existential hypothesis. Here we enrich the conceptual system of science by the introduction of entirely different factors, radically new as compared with those embodied in previous levels of description or explanation. The introduction of the concepts of the electric and magnetic field may serve as a good example. Mechanical phenomena such as the acceleration of test bodies, or the deflection of compass needles, may provide the evidential basis for the inference of certain electric or magnetic field conditions. The concepts of electromagnetism are however not explicitly definable in terms of mechanical concepts, nor are the laws of electromagnetics logically deducible from the laws of mechanics.

It is equally true that the concepts of neurophysiology are not explicitly definable in terms of the concepts of molar behavior. If hypotheses concerning the structure and the functions of the intradermal constituents of organisms were to be constructed merely on the basis of the peripheral and behavioral evidence, there would be such a bewildering multiplicity of alternatives as to render all theorizing of this sort hopelessly indefinite. But, as I indicated before, the situation in psychology is in many ways much less problematic than that of theoretical physics. Anatomy and physiology are well developed and thriving sciences with their own basis of observation.

These disciplines have already provided many fruitful specifications of concepts which are defined only in outline and in a promissory fashion by molar behavior theory. It would seem that the time has come for this sort of "Anschluss." Analogical or abstract models of the Freudian or Lewinian type will from now on have to be appraised in terms of the isomorphism they attain with the corresponding structures and processes certified by the physiological approach. There is no question that the mythological fictions as well as the hydraulic models of psychoanalytic theory have been immensely fruitful heuristic devices. Similarly, it may be admitted that Lewin's topological and field theoretical concepts have stimulated not only a certain way of talking in the psychology of motivation but also a great number of highly original and significant experimental investigations. But from a critical methodological point of view there are serious doubts as to whether these types of theorizing amount to more than a tentative groping for the laws and concepts which will ultimately have to be provided by the molecular approach. There are, again, significant analogies between the Freudian speculations and that of the various fluid hypotheses in the now obsolete physical theories of heat and electricity. And the tension systems of Lewin display a suspicious similarity to the theories of the elastic ether in the now abandoned picturizations of Faraday, Maxwell, and Kelvin. If Lewin's mathematical vector and field theories did not suffer from grave internal shortcomings, they could at least be considered as formal axiomatizations of molar concepts. In that case his concepts would be methodologically on a par with Hull's intervening variables, especially as they are interpreted by Spence. But not all is well with the internal consistency and the operational anchoring of the topological and field theories.[42]

The methodological comparisons we have made between psychological and physical theories indicate, I think, fairly clearly that the success of typically nonphysiological theories of behavior is bound to be severely limited.

Permit me a few more general remarks by way of conclusion. Philosophers and psychologists have for a long time agreed in their characterizations of the subject matter of psychology. No matter whether they started from introspectionist or from behaviorist prepossessions, the features of *memory* and *purpose* have always been central in these definitions. In order to do justice to the psychology of the "higher" processes in humans they may have added the feature of *awareness* or *consciousness,* or its behavioral equivalent, the capacity for *symbolic responses* of certain types. With the mnemic and teleological characteristics as basic, the continuity of psychology with biology has been, or at least should have been, beyond question. The laws of behavior on the molar level of analysis formulate in various ways the features of docility and of the persistence in the pursuit of goals. Because of their distal or peripheral character these laws, even if to a lesser degree than those of the ecological approach, are inescapably statistical. Internal variables remain largely unspecified and uncontrollable. They also leave room for vicarious functioning or substitutibility of the mediating central processes. It is precisely this characteristic which mnemic and telic processes have in common. The recent contributions of cybernetics and of analytical biology [49, 50, 55, 64] have shown clearly that the metaphysical quest for *teleological explanations* must—and can—be replaced by the scientific search for *explanations of teleology.* The phrase "telological mechanisms" is significantly and fortunately no longer a contradiction in terms. With the new cybernetic neurophysiological explanations of memory, of purposive activity and of symbolic communication, a new and higher level of explanation has at last been attained.

We have seen that psychology in her sibling rivalry with the older science of physics has become increasingly self-conscious in her attempts at emulating the manner, if not the matter, and the achievements of advanced physical theory. But if psychology according to the more customary delimitations of its subject matter is not, and cannot be, a fundamental science, then it is only apt to suffer from those quixotic ventures. It will thrive only if it either reconciles itself to the limitations imposed by its characteristic level of analysis—or else if it abandons its often so jealously guarded identity and merges with the more fundamental disciplines in the concerted attack on all the surrounding problems. From the point of view of the unity of all sciences, we can acknowledge only a practical division of labor between the behavioral, the physiological, and the physical disciplines. If psychologists insist on limiting their endeavors to the molar approach, they can attain a certain close-to-life type of empirical laws. But if they wish for the larger synthesis of the more comprehensive explanations, if they strive for the greater deductive power of a more unified theory, they will have to be much more than psychologists.

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